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THE PSYCHOLOGICAL REVIEW

TOWARD A DYNAMIC PSYCHOLOGY OF COGNITION[1]

BY EDNA HEIDBREDER

Wellesley College

I have decided to use this hour to present a hypothesis that has been haunting me for nearly ten years. During that time I have been working on experiments on the attainment of concepts. Persistent regularities in the data suggested a hypothesis that seems not only to make intelligible the outcome of these particular experiments, but to be a possible means of tracing a meaningful order throughout the general field of human cognition. Thus, the hypothesis is derived only in part from the data from my own experiments. In its entirety it is an interpretation of facts drawn from a much larger field, liberally supplemented with speculation. Naturally I emphasize the point that the interpretation is *merely* hypothetical—that it is offered as highly tentative but perhaps suggestive, and in that sense possibly useful.

I. THE HYPOTHESIS IN OUTLINE

In its broadest outlines, the hypothesis may be stated very briefly. It maintains that all human cognition, taken psychologically—*i.e.*, as reactions of living organisms—may be ordered with respect to two kinds of performances: the perception of concrete objects and the attainment of concepts. Expanded a little, but still blocked in roughly, it may be presented as involving three major claims:

I. The first is that the *typical* and *dominant* cognitive response in human beings is the perception of concrete objects; that all other cognitive responses may be regarded as in some sense approximations to or modifications of this form.

II. The second is that the *distinctive* cognitive response in human beings involves the attainment of concepts and is itself a modified form of the perception of objects.

III. The third is that with respect to these two modes of reaction,—one dominant, the other distinctive,—it is possible to *order* the other cognitive reactions in such a way as to show meaningful lines of relationship among them. Primarily this order is determined by the function performed. The relevant question is: What is the organism doing? Is it operating chiefly with objects or with concepts? But since cognitive activities have presumably evolved along with other biological activities, it may be expected that this functional order is not unrelated to the phylogenetic sequence; that the dominant and typical mode of response is older and more deeply em-

[1] This paper is a somewhat lengthened form of the presidential address read at the annual meeting of the Eastern Psychological Association at Boston University on April 8, 1944. The increase in length is due chiefly to the addition of footnotes—some of them rather long—but partly to the insertion of sentences here and there into the text. A few changes have been made in the wording of the paper as read.

bedded in the organism, the distinctive mode a later arrival and less solidly established. It is assumed too that learning is importantly involved in both these forms of human cognition.

II. DEFINITIONS

Obviously several of the terms just used require definition.

To say that the perception of concrete objects is the *typical* reaction in human cognition is to say that human beings in all their other cognitive responses are in some measure doing what they do in perceiving objects—that other modes are in some sense continuous with this typical form; as modifications of it, as approximations to it, as potential constituents of it, or as developments and extensions beyond it.

To say that the perception of objects is *dominant* in human cognition is to say that it is the kind of cognitive reaction most readily aroused; the kind most likely to occur in ordinary conditions of stimulation; or rather, since living organisms do not produce reactions, cognitive or otherwise, in neat single file, that the perception of objects is likely to provide the center or centers about which cognitive activities are organized. To call this response dominant is to say that the chances are that on any occasion the human cognitive reaction will resemble, as closely as the conditions of the activity permit, the kind of response that occurs in perceiving concrete objects.

To say that the *distinctive* mode of cognition in human beings involves the attainment of concepts is to say that the quantitative differences between human beings and other animals are so great in this respect that conceptual reactions may be regarded as the differentiae of human cognition. Specifically it implies that they are correlated with those human achievements—such as the arts and sciences—which other animals do not attain. It is an important point in the present hypothesis, that the distinctive cognitive response in human beings is *not* the dominant form.

To say that all forms of human cognition can be *ordered* with respect to cognitive responses of two kinds is not to say that the whole field of human cognition can be exhaustively explained. It would be unfortunate if the hypothesis aroused any such expectation. What it proposes to do is something far less staggering to contemplate, namely, to trace an order through cognitive reactions about which very little is known, in the hope of gaining clues that may lead to further knowledge.

But the two chief points of reference still remain undefined, the concrete objects and the concepts with respect to which the key modes of cognition have themselves been identified.

By a *concrete object* is meant a material body in the external world, typically visible, tangible, and manipulable. Concrete objects are taken, in the first instance, as unreflectively and uncritically as possible—as the 'things' of common sense, undebauched by learning of any sort, physiological or psychological, physical or metaphysical. Objects, so regarded, are selected as the first point of reference. The psychological processes involved in the perceiving of objects are themselves identified, and even defined, with respect to such objects—more precisely with respect to observed reactions to such objects. They are inferred from behavior. It is assumed in this hypothesis that all perceptual reactions are reactions of living organisms, and the perception of objects is defined as consisting of *whatever* reactions turn out upon suitable investigations to be involved in specified behavior, related in specified ways to specified objects. Every psychologist knows that in actual practice such investigations are

enormously complex. In principle, however, they consist in discovering the precise conditions in which selected kinds of behavior occur, in determining the precise relations between observed reactions and selected features or aspects of the experimental situation, and in constructing the precise determinants which must be inferred if the behavior is to be made intelligible.

It will be observed that, in selecting concrete objects as its first point of reference, the hypothesis starts, as all scientific inquiry starts, with the prescientific—with the world of ordinary experience and observation, simply accepted as given, from which portions or aspects are singled out as points of departure for investigation.[2]

The word *concept*, throughout this paper, denotes a logical construct which, by means of symbols or signs, may be used interpersonally. Just as many people can perceive and use the same object—say a table—many people can attain and use the same concept—say twelve—and the use of the words 'the same' is justifiable on similar grounds in both cases. It should be noted especially that the word 'concept' denotes a logical construct, not a psychological process or event, and that it is not used in this paper, as it sometimes is, to denote an idea, considered as a psychological structure or process. It is assumed, however, that a person attains a concept only by means of some kind of reaction of his organism. To denote such reactions—the actual psychological processes by which an organism attains concepts—the terms 'conceiving,' 'conception,' and 'conceptual reaction' are used. Such processes are defined, in a manner analogous to that used in defining perception, as *whatever* reactions turn out upon investigation to be involved in the behavior by which concepts are attained.[3] Concepts them-

[2] It may be well to emphasize the point that this hypothesis deliberately begins *in medias res*, setting aside for the moment many important problems and turning aside from other possible starting points; that it finds its point of departure in a world admittedly unexplained, which in its frame of reference it simply accepts, and which for lack of a better name may be called the world of common perception. It is hardly necessary to say that a psychology of cognition is under no obligation to account for such a world epistemologically or metaphysically; what is here insisted upon is that this world is initially taken as unexplained psychologically. But in initially accepting this unexplained world, there is no intention of disclaiming as a part of the task of a psychology of cognition that of accounting psychologically—*i.e.*, in terms of the reactions of human organisms—for its characteristics as perceived and otherwise known. Investigation starts from this world in order to return to it, the richer for its journey. It would be instructive to supplement the more conventional kinds of psychological research on perception, by discovering to what extent this world of common perception is common to different cultures as well as to different individuals. Is it true, for example, that people of all cultures perceive objects? They seem to do so, though the fact ordinarily emphasized by students of culture is that the same (or similar) objects seem to be differently perceived in different cultures. Köhler has an interesting discussion of cultural differences in perception (33). By comparing the perceptual reactions of members of different cultures, much might be learned about the psychological reactions involved in perceiving objects—not only about reactions characteristic of different cultures, but about reactions which may be common to members of many and perhaps all cultures, and which escape notice and report, possibly because they are so basic that they are taken for granted.

[3] This definition of conceptual reactions accounts for the use of the word 'attain' in this paper. That word was selected as being relatively empty and colorless; as affording a means of saying that a person had reached—attained—a point (*e.g.*, in the course of an experiment, or of his development, or of an argument) at which he exhibited behavior indicative of a concept, and of stating that bare fact without implying anything about the actual processes by which he had arrived at that point or maintained his position there.

selves, like things, are here regarded as interpersonal and public.[4]

In the first instance, concepts are inferred from social behavior—from such social practices as communication through language, to take the most obvious example. With Reichenbach, the present hypothesis holds that "Every theory of knowledge must start from knowledge as a given sociological fact" (47, p. 3). In actual practice, the involvement of particular concepts in behavior is rather definitely determinable. In a manner closely resembling that in which experiments on perception are conducted, an investigator, by setting up and controlling conditions with respect to specified concepts, can discover whether or not a subject produces behavior from which a specified concept can be inferred as an essential determinant; in what conditions he produces such behavior; and even in what measure, if the specified concept is not indicated, the subject's behavior approximates its attainment.[5] By discovering more and more about the conditions in which specified concepts are attained, an experimenter provides himself with increasingly adequate grounds for the construction of the psychological processes assumed to be involved.

And now it must be admitted—indeed it must be apparent from these preliminary statements—that the hypothesis is not only tentative but unfinished and roughhewn. It is miles away from those precisely articulated structures of pared-down sentences that contemporary builders of psychological theory have set up as models. Still, it is offered in the hope that even a crudely carpentered structure may have its uses, and in the belief that, in some stages of investigation, such structures are appropriate and in fact indispensable.

III. A Biological Analogy

To put the whole case as concretely as possible, the hypothesis treats human cognition as part and parcel of the way of life of a species of living creatures—as one of the ways in which human beings, as biological organisms, react adaptively to their environment. Specifically, it maintains that human beings, in their present stage of development, are so constituted and conditioned, that in ordinary circumstances their most probable cognitive response is that of perceiving objects; and that if they are aroused to cognitive activity in conditions in which the perception of objects is impossible or inadequate, they are likely to make a response which departs from that mode of reaction as little as

[4] Perhaps it should be stated explicitly that concepts, like concrete objects, are simply 'taken'; that the question of their 'reality,' of their epistemological and metaphysical status, does not arise in psychology; that just as a psychologist may use concrete objects in his experiments without inquiring into their reality, so he may set up his experiments with respect to selected concepts without reference to the problems posed by realism, nominalism, and conceptualism. Psychologists are extremely likely to take this point for granted as it concerns concrete objects—*e.g.*, the stimuli and other apparatus they use in experiments on perception, and the mazes, puzzle boxes, and conditioning apparatus they use in experiments on learning. They are not nearly so likely, however, to accept the point unhesitatingly with regard to concepts, though the metaphysical and epistemological problems concerning objects are no less genuine as problems, and no nearer solution than those concerning concepts.

[5] The experiments of Hull (26), Kuo (36) and Smoke (52) give clear-cut illustrations of this procedure with adult human subjects. Welch (56, 57) has used somewhat similar procedures in studying intellectual reactions in very young children. Experiments with animals show particularly ingenious devices for the presentation of 'tasks' from the performance of which specific determinants of behavior can be inferred—*e.g.*, Hunter's delayed reaction experiment and temporal maze (27, 28, 29); Klüver's method of equivalent stimuli (30); Krechevsky's scheme for studying 'hypotheses' in rats (34, 35).

the conditions allow. Cognition is treated as a biological activity analogous to other biological activities—for example, respiration. Like respiration, it has its typical and variant forms. If for the moment the word 'breathing' is used to denote the most common form of respiration—the fairly regular pattern of inspiration and expiration that goes on in ordinary circumstances in a healthy, undisturbed organism—then breathing may be regarded as the typical and dominant form of respiration. But breathing is by no means the only form respiration may assume. There are many forms that vary widely from breathing—gasping and panting, snoring and sighing, laughing and sobbing, to name only a few. These, to be sure, are often regarded as disturbances rather than modifications of breathing; but insofar as they are regular and recurrent ways in which respiration goes on under specific conditions of stimulation, they are roughly analogous to the forms of cognition—remembering and imagining, for example,—which the present hypothesis treats as modifications of the perception of concrete objects.

The analogy is enlightening even at the point where it most conspicuously breaks down. There are no higher forms of respiration comparable with the higher or conceptual forms of cognition. To be sure, there are highly developed respiratory patterns in speech and song, and perhaps the exercises of the Yogi should not be forgotten. But these skills, however importantly they involve respiration, do not extend and refine the function of respiration as such. Speech and song do not effect an interchange of gases in a manner unattainable by ordinary breathing. But the function of conceptual reactions is precisely that of extending and refining the cognitive functions *as cognitive*. They perform feats of cognition unattainable by perception, and thus give human cognition its distinctive character.

But analogies are too tricky to be given free rein. After all, the hypothesis must be considered literally and directly. Its specific claims must be examined more closely.

IV. THE PERCEPTION OF OBJECTS

I. The first point is that in perceiving concrete objects, human beings are performing their dominant and typical cognitive response.

a. The perception of objects as dominant.—Clearly, the hypothesis does not begin at the beginning. No psychologist need be reminded that the perception of objects is far from simple and by no means primitive. Indeed, part of what is meant by calling this response *dominant,* is precisely that a human being is *more* likely to make this highly complex, highly developed response than the simpler and more primitive reactions he has at his disposal—or rather that these simpler and more primitive responses are likely to be caught up in the organization involved in perceiving objects, either to be incorporated into that reaction as components or to be related to it in such a way as to give it background.

It is especially suggestive to reflect that an ability to perceive objects presupposes an advanced position on the phylogenetic scale. Without distance receptors such a response, as here defined, would be impossible. And what is more, the utility of such a response seems directly proportional to status on that important stretch of the phylogenetic scale in which manipulation becomes increasingly effective in supplementing locomotor adjustments to the environment, eventually even outranking locomotion as the characteristic mode of reaction. In the light of what happens at different stages of evolution, a close relation between cognition and

motor capacity is strongly indicated. The function of cognition then appears as that not only of sensing selectively the biologically useful aspects and features of the environment, but of responding in such a way that sensory reaction is adapted to the motor responses of which the organism is capable. The perception of objects thus appears as that kind of organization of sensory reaction which is especially adapted to the motor capacities of a highly manipulative animal; a mode of reaction in which stimulation from various receptors gets organized in such a way that the organism perceives functionally significant units, compact, delimited, and salient—literally outstanding—suitable to the stimulation and control of motor reactions, especially manipulation.[6] According to the present hypothesis, this mode of cognition is now dominant in man; and its dominance is correlated with the fact that in his motor behavior, man responds to the environment not merely by moving about in it—not merely by pursuit and withdrawal—but by doing something to it and with it, altering it by manpulation.

To be perfectly specific, the hypothesis means, in calling the perception of objects dominant, that under ordinary conditions, a person is more likely to see an apple than redness and roundness; more likely to see tables and chairs, on a floor, in a room, than a patterned arrangement of colored figures and ground; more likely to see a stable road on which his car and other cars are moving, more likely to see stable trees and filling stations in a stable landscape through which he is passing, than swirling masses of color and form. In other words, human beings are likely to respond at once with *more* than sensory reaction, with more even than the perception of sensory patterns. In all literalness, they are likely to see more than meets the eye; they are likely to perceive concrete objects—*things*.

That human beings are extremely likely to perceive *things* is not news. That fact forced itself obtrusively upon the first experimental psychologists as they struggled with the stimulus error. The point here emphasized is that a struggle was necessary; that a person who set out to observe something simpler than an object—some sensory quality or intensity perhaps—all too often found himself, despite training to the contrary, making a far less simple response which occurred far more readily; he found himself perceiving *things*. Similarly, facts which the Gestaltists have pointed out about the perceptual field may be ordered with respect to degrees of dominance among perceptual reactions. Immediately figure-and-ground suggests itself as an illustra-

[6] The evidence here is of course partial and indirect. It has long been recognized that the differentiation of motor organs precedes that of sense organs, and there is no doubt of the increasing proficiency through the animal series of manipulatory behavior, which reaches its highest development in man as locomotor behavior does not. But sensory capacity as such does not increase in the same way; even detail and pattern vision seem to be effective far down the animal scale and there is no evidence that it is correlated in effectiveness with evolutionary level. It must be remembered, however, that such sensitivity would presumably be useful in guiding locomotion as well as in controlling manipulation. It is especially interesting that stereoscopic vision, directly involved in the perception of objects as here defined, seems not to occur below the primate level, since the physiological structures presumably necessary for such vision is not found in animals below the primates. Besides, there is indirect evidence for increasing organization and integration of the sensory activities among themselves and with other activities—which is after all the important point from the standpoint of developing perceptual organization rather than increasing sensory proficiency—in the increasing encephalization and corticalization of behavior through the animal series. Recent summaries of the relevant evidence are available in books by Morgan (43) and Munn (44).

tion; with objects, the most prominent of figures, corresponding to the dominant perceptual response, and with ground, not at all thing-like, corresponding to the least dominant of perceptual reactions. Indeed an arrangement of figure and ground in which figures vary in degree of prominence and in which objects are the salient figures, might readily be interpreted as an organization of the perceptual field especially adapted to the motor capacities of animals living by both locomotion and manipulation and given especially to the latter. Conceivably ground might be the perceptual correlate of an older but now less dominant mode of motor reaction, locomotion. Even the fact that ground may itself become figure may be taken as indicating that whatever the organism cognizes directly is likely to arouse a response as much like that of perceiving objects as the conditions of stimulation permit. In general, it is possible to claim a positive correlation within the perceptual field between the degree of dominance of perceptual responses and the degree to which what is perceived is a useful cue to manipulation; and this order in turn may be positively correlated with 'thing character' in Koffka's sense of the term (31, pp. 70–72). Of course these suggestions are *merely* suggestions. Their primary purpose is to show along what lines the present hypothesis would direct inquiry in ordering perceptual responses which, as compared with the perception of objects, seem less dominant.

b. The perception of objects as typical.—To say that the perception of objects is *typical* of human cognition is to say considerably more than that it is dominant. It is to say that there is nothing in cognition which is not in some sense present in this mode of perceiving. The selection of a type—as of any point of reference—is always a matter of convenience, and here convenience is determined by the fact that the perception of objects, still firmly centered in sensory stimulation, nevertheless shows unmistakable signs of elaboration upon and beyond sensory reaction. Two important cognitive functions here merge and coöperate: (1) sensory responsiveness to stimulation, predominantly environmental, and (2) organization and supplementation of the responses to such stimulation in ways numerous, various, and vastly complex.

No psychologist pretends to be able to describe exhaustively what happens when a human being perceives a concrete object. Still, some statements about such an event would win assent from most psychologists. Doubtless all would agree that sensory stimulation is directly involved in true—*i.e.*, non-hallucinatory—cases. They would also agree that sensory stimulation is not enough—that more is included than the stimulation of one or even of two or many receptors; they would agree that somehow an integration is effected, though they would disagree on the manner in which it is brought about. They would probably agree too—despite differences in interpretation—that concrete objects are perceived as in some way bounded and formed, and as characterized within limits by what the Gestaltists have named the constancies. No psychologist, not even the most ardent nativist, would deny that learning figures largely in perceiving objects—that what a man perceives is strongly determined, and intimately and complexly determined, by what he has learned. Neither would anyone deny that perceiving an object is a flexible and fluctuating affair, greatly influenced by needs, attitudes, and interests—by sets transient and enduring—in short by what the organism is doing or is likely to do about it. From this point, it is only a step to the statement that a per-

ceived object is an organized cue to action, and that its functional significance is the core of its organization. In sum, the perception of objects is a mode of reaction in which a human organism, in the very act of receiving stimulation, reworks it and adapts it to its own needs, capacities, and enterprises.

But there remains an important fact about the perception of objects which, though implied in much that has been said, has not yet been stated with sufficient prominence. Bridgman refers to it in *The nature of physical theory* when he discusses the difficulties occasioned by language. He says: "to say 'I see a horse' gets recognizably closer to the direct experience than to say 'There is a horse,' for the first describes my experience as an activity, whereas the second freezes the activity and substitutes for it something static, something which did not occur in direct experience and something which itself constitutes a human invention" (**5**, p. 17).

The point here emphasized is that the kind of perception expressed in the sentence "There is a horse" is an event involving a construct upon and beyond sensory data. The perception of an object, closely implicated in sensory stimulation as it is, involves a construct not itself exhibitable to the senses. *How* the construct is achieved is not the question at present. The important point is *that* it is achieved.

It is interesting that Bridgman goes on to say that "it requires a certain amount of sophistication to feel the difference between saying 'There is a horse' and 'I see a horse'" (**5**, p. 18). In effect, he says that to anyone naively and uncritically perceiving, the horse is simply there; and that only after reflection does the person discover that the reaction involves more than is 'given.' The construction, however made, occurs as automatically as that. This the present hypothesis asserts in calling the response dominant. But it goes further and proposes that this reaction be taken as typical; that this reaction of perceiving a thing, at once naive and elaborate, be made the focal point in an account of human cognition. Already in perceiving a thing, a person has gone beyond data to a construct. Hence human knowing, according to the present hypothesis, is centered not in pure data but in elaborated data. Indeed it is just the interpenetration of data and construct, of sensitivity and elaboration, that gives the perception of concrete objects its suitability as a primary point of reference—that justifies its selection as typical. It is in this sense that the perception of objects contains in some form and in some measure everything that is found elsewhere in human cognition.

It has already been said that there are some kinds of perceiving which may be called sub-dominant—the perceiving of ground, for example, and of patterns and figures that lack full thing-character. These same reactions may now be described as sub-typical and pre-typical—as simpler and more primitive than perceiving objects, as forms of cognition in which the organization of sensory reactions has not become so largely constructive as that which characterizes the perception of objects. Still farther away in the direction of the pre-typical is the kind of sensing that occurs in simple sensori-motor reactions. These may be regarded as borderline cases between the cognitive and the pre-cognitive, since in such cases the sensory reaction often remains outside the cognitive organization altogether. All these reactions which to a greater or less degree fall short of the typical, are of course regular and fundamental modes of human cognition. They are in fact more fundamental than that selected as typical in the sense that they are phylo-

genetically its predecessors and potentially its constituents.

Departures from the typical also occur, of course, in the opposite direction —that in which the constructive and elaborative activities are more rather than less developed, and functionally more significant. Remembering and imagining may be regarded as modifications of the kind of activity that goes on in perceiving objects—as modifications in which the external stimulation is functionally less central and in which the organism's own contributions—its needs, its interests, its attitudes, its habits, its going enterprises—are relatively more decisive. Both afford interesting borderline cases. When a person sees a familiar object or scene, is he chiefly perceiving or recognizing? And is a person chiefly perceiving or imagining when he takes the Rorschach test or the T. A. T.? Bartlett's *Remembering* (2) is full of cases of both remembering and imagining, interpreted as constructive activities showing the same pattern as perceiving, which is itself treated as largely constructive. But again, it is possible at present to indicate only roughly the direction of inquiry the hypothesis suggests. And of course it must not be forgotten that remembering and imagining occur in numerous and various ways, some of them so elaborate and so largely constructive that they can scarcely be considered apart from conceptual reactions. But in the attainment of concepts there are indications of a kind of construction so revolutionary that it must be considered as a special topic.

V. The Attainment of Concepts

II. This brings the discussion to the second main point in the hypothesis: that the distinctive cognitive activity in human beings involves the attainment of concepts, and that conceptual responses may themselves be regarded as modifications—but very wide modifications—of the perception of objects.

The word 'distinctive,' it will be remembered, is not intended to imply that nothing like conceptual reaction occurs in non-human animals.[7] Neither does it imply that in human beings such reactions form a class apart—occurring separately from, or on top of, other cognitive reactions. On the contrary, the hypothesis assumes that the many kinds

[7] The well-known investigations of apes by Yerkes (60, 61, 62) and Köhler (32) immediately suggest themselves, and Klüver's (30) cautious interpretations of the behavior of monkeys in experiments on the equivalence of stimuli are also relevant. Claims have been made, too, that animals farther down the phylogenetic scale are capable of activities related to conceptual processes—*e.g.*, abstraction, generalization, symbolic reaction and even reasoning. Fields (10, 11), for example, claims, on the basis of his experiments, that rats and raccoons respond to the concept of triangularity, but Smith (51) who obtained comparable behavior in cats believes that the evidence does not justify the claim that concepts are involved in such behavior. Maier (41, 42) offers evidence which he interprets as indicative of reasoning in rats. The delayed reaction experiment and the double alternation experiment have been used with various animal forms, sometimes with success in obtaining the 'correct' reaction. According to Lashley "there is some reason for believing that generalization is one of the primitive, basic functions of organized nervous tissue" (39, p. 302). The whole field is controversial, partly because of lack of agreement in the use of such terms as generalization, abstraction, and conception. The question is not whether or not animals attain concepts—a point which cannot be settled until agreement is reached as to the meaning of the term 'concept'—but whether or not animals produce behavior approximating in specifiable ways that which in human beings is regarded as involving the use of concepts. There is little doubt that some infra-human animals are capable of such behavior, and that conceptual activities of human beings are not unrelated to, and not altogether unlike, kinds of behavior which can be identified in those animals as determined by relations among, or by selected aspects or details of, presented stimuli, or by symbolic processes in the absence of stimuli.

of cognitive reactions, like other activities of living organisms, mutually modify and influence one another. It maintains that the influence of conceiving is pervasive, that human perception, for example, is different from what it would otherwise be, because it occurs in organisms in which conceptual reactions have become relatively important. A person may react mainly conceptually, or mainly perceptually, but at no time is his reaction purely or merely of one or the other sort—or for that matter, purely or merely cognitive.

In conceptual behavior, it has just been said, there are signs of changes in mode of organization so great that they may be called revolutionary. They are revolutionary in the sense that what is organized does not constitute a manipulable thing. Conceiving terminates in a construct suitable to another mode of reaction, the symbolic. If perceiving an object is a mode of cognitive organization adapted to the motor, and especially the manipulatory capacities of the organism, attaining a concept involves a mode of organization adapted to the reactive capacities of an organism not limited to direct motor responses to the environment.

When an organism reacts conceptually, it operates at some remove from the perceptible world to which it can make direct motor reactions. To attain the concept 'red' for example, a person must have had several encounters with light of a given wave length; somehow the responses so stimulated must have been separated or abstracted from their various contexts, and somehow they must have been brought together or generalized. There are indications that, as in the perception of a thing, both selection and integration have been effected, but not, as in the perception of a thing, on the basis of togetherness in space and time, not in such a way that a localizable, palpable, manipulable unit is achieved. Instead, a unit is attained which is abstract and general. The basis of selection is narrower and the range of application is wider. In conception, the function of perception is both refined and extended, and at the same time the pattern of perceiving an object is preserved in the sense that the outcome of the activity is a unit, integrated and delimited; a construct usable, though indirectly, in action. A concept refers to *more* than a thing and *less* than a thing—to more and less than any particular, concrete situation. If the concept 'red' is applied to a particular concrete object, say an apple, it refers to a single selected aspect of the apple and also beyond the particular red and the particular apple. The case is no different if the noun 'apple' is taken as an illustration instead of the adjective 'red.' The concept 'apple' refers to a limited set of characteristics of the particular concrete object to which it is applied and it also refers beyond that object. Even such concepts as 'everything' and 'universe' refer to selected aspects of, as well as to extensions beyond, perceived things and perceived situations. The attainment of concepts like 'apple' and 'red' involves relatively little elaboration beyond that present in perceiving objects, but it does involve the important step of departing from perceived objects, of selecting from them, extending and elaborating beyond them, and so of arriving at a mode of behavior that operates with constructs which are abstract and general.

In the foregoing account, the words 'abstraction' and 'generalization' have been used, and also such words as 'selection' and 'construction,' 'elaboration' and 'organization.' It is of the utmost importance to say as plainly as possible that these words are not intended as descriptions of psychological processes, or even as names of specifiable kinds of psychological processes. The psycho-

logical processes are inferred, not observed; they are inferred from behavior and the results of behavior. It is the constructs, the concepts, that have characteristics called general and abstract, and it is assumed that these constructs are somehow attained by activities of the organism. Such words as abstraction and generalization refer to *whatever* processes turn out upon investigation to bring the organism to the point of operating with abstract and general constructs; they do not characterize those processes descriptively. In any given instance in which a person is said to be using concepts, the concepts, together with the psychological processes involved in attaining them, are inferred from that person's behavior. To be unpardonably specific, and even more tedious than may be strictly necessary, the investigator, the psychologist, infers that a person is using a concept—say triangle—when that person, confronted with a succession of suitably constructed and varied situations, consistently reacts in a specified manner to those situations to which the experimenter's definition of the concept, triangle, is applicable, and *not* to those to which the experimenter's definition of the concept, triangle, is *not* applicable. Even then the experimenter is not justified in inferring that the psychological processes by which the concept is attained and used in the person under observation, resemble the psychological processes occurring in the experimenter himself when he attains and employs the concept in question. On the contrary there are good grounds for believing that operations with the same concept may involve different psychological processes in different people, and in the same person on different occasions.[8] By appropriately varying and controlling the experimental conditions, the experimenter may come closer and closer to reconstructing those processes—he may tell more and more definitely what the organism is reacting to; he may establish more and more perfect correlations between specified reactions and the conditions in which they occur. In practice it is a delicate and intricate matter to set up a situation appropriate to the investigation of conceptual behavior, but probably no more delicate and intricate than to devise means of investigating perception. It may even be less so. At any rate the principle involved is sufficiently plain. It is essentially the same as that used in objective investigations and interpretations of sensory and perceptual reactions, such as those of Boring (4) and Stevens (53, 54), Lashley (37, 38) and Klüver (30), Tolman (55) and Brunswik (7, 8).

But to return to the main thesis:

According to the present hypothesis, an organism in attaining a concept performs, at a level beyond the perceptual,

[8] The experimental evidence on this point is both abundant and various; a few illustrations must suffice. Hanfmann and Kasanin (18) show that the same concept may be attained by very different routes, traceable through the subjects' overt behavior and indicated also by their verbal comments. The same fact, again indicated by the course of overt performances and also by verbal comment, was indicated in experiments by the present writer (19, 20, 21) and in one of them led to the comment, "Most of the words used in describing thinking . . . , do not refer to processes but only to results produced. . . . There is no process of analysis, for example, but a number of processes which differ widely in nature, though they produce a common result" (19, p. 173). Different psychological routes to the same concept are often suggested by definitions given by subjects upon the attainment of a concept (26, 52). Those who prefer detailed, descriptive reports by trained observers will find relevant evidence in the systematic program of research conducted at Cornell. The investigations perhaps most closely related to the present problem have been listed (9, 40, 46, 47). The frame of reference for these studies was provided by Bentley's *The new field of psychology* (3).

a cognitive function very similar to that it performs when perceiving a concrete object. Nothing indeed is more trite than the statement that conceiving carries the function of cognition a step—or many steps—beyond perceiving; freeing the organism from what is immediately given, here and now, enormously increasing both the refinement and the range of its operations, enabling it to work symbolically with the absent and the distant, with the past and the future, and to go beyond the actual and construct the merely possible. All this the present hypothesis accepts and even emphasizes. But it also maintains that in all these operations, the organism is doing essentially what it does in perceiving an object; that again the organism reacts by selecting and integrating, again arriving at a compact and delimited unit, but now one that can be understood and utilized symbolically, at some remove from the immediacies of the perceptible, manipulable world. And in the present hypothesis it is suggested that this mode of operation is derived from and is a development of that involved in the perception of concrete objects; that the perception of things imposes something of its nature on the highest reaches of symbolic thought. A symbol itself is an object having more or less thing-character. A printed word is a 'mound of ink.' It becomes a symbol when it is caught up in a larger organization, in which the functional center is shifted farther from the concrete and receptive, farther toward the abstract and constructive.

But if conceptual reactions perform functions unattainable by perception alone, they also have their limitations, and these, too, are instructive. From one point of view, conceiving may be regarded as frustrated and sublimated perceiving—as a cognitive activity that never touches the actual object or situation with which it is concerned. To act conceptually is to operate indirectly, always at some remove from the perceptible and the palpable. To many people, this state of affairs seems unsatisfactory; cognition does not seem complete unless it terminates in perceived objects or situations—or at least in objects and situations conceivably perceptible. To them, a cognitive enterprise that terminates in symbols seems not to have reached its proper goal. Many people, for example, are dissatisfied with scientific explanations in terms of mathematical equations; they prefer mechanical models. To many psychologists, picturable operations of physiological structures, even if they are hypothetical, seem a more satisfying explanation than statements of correlation. These facts are of considerable significance to a hypothesis that regards the perception of objects as the dominant and typical cognitive response.

A passage from Skinner's article on the concept of the reflex illustrates this point admirably. The author, having asked and discussed the question "What is the flexion reflex?" says:

> We have been proceeding, of course, upon an unnecessary assumption, namely, that there *is* a flexion reflex which exists independently of our observations, and which our observations approximate. Such an assumption is wholly gratuitous, but it is *remarkably insistent*. . . . If we remain at the level of our observations, we must recognize a reflex as a correlation. But the *immediate uncritical reaction* to a definition on that basis is that a correlation, in point of satisfaction, *is not enough*. There is an *urge toward solidification*, clearly evident throughout the history. We *turn instantly* to the reflex arc for *material support*[9] (**50**, pp. 449–450).

Here is a statement which, translated into the terms of the present hypothesis, says that the distinctive cognitive reaction in human beings is on a precarious

[9] Italics mine.

footing. In point of satisfaction, it is not enough. It must make its way against a reaction, presumably more dominant—in this case that of turning to an object, a thing, the reflex arc.

Skinner's account is especially interesting in that it traces historically a preference for the conceivably perceptible even in conditions in which actual perception has been impossible. For here the choice is not between on the one hand actually perceiving an object, a reflex arc, and on the other operating with a construct, a correlation. It is between using concepts of different degrees of abstractness; of different degrees of remoteness from observable concrete objects. And a construct of a picturable object, though such objects have never been observed, has been persistently preferred to a construct which takes into account precisely the relevant abstract relationships and nothing more. Apparently it has been difficult *not* to respond with more than is strictly necessary. Apparently the immediate reaction has been to operate with a surplus, with a construction which is both logically superfluous and empirically redundant.

The use of surplus constructions of this sort implies a kind of psychological reaction of considerable importance to contemporary criticisms of knowledge. In one sense, human liability to such reactions is the psychological *raison d'être* of such disciplines as operationism, logical positivism, and recent studies in semantics. All of these include warnings and propose safeguards against just such reactions—against reification, against hidden perceptual references in conceptual structures, against uncritically treating constructs as if they referred to existential realities, physical or metaphysical. Bridgman says, for example, of the concept of the electric field:

> In the first place, an examination of the operations by which we determine the electric field at one point will show that it is a construct, in that it is not a direct datum of experience. . . . Next, from the formal point of view of mathematics, it is a good construct. . . . Now nearly every physicist takes the next step, and ascribes physical reality to the electric field, in that he thinks that at every point of the field there is some real physical phenomenon taking place. . . . At first this view most naturally involved as a corollary the existence of a medium, but lately it has become the fashion to say that the medium does not exist, and that only the field is real. . . . This view is usually credited to Faraday, and is considered the most fundamental concept of all modern electrical theory. Yet in spite of this, I believe that a critical examination will show that the ascription of physical reality to the electrical field is entirely without justification. . . . There can be no question whatever of the tremendous importance of the concept of the electric field as a tool . . . ; electrical science is inconceivable without this or something equivalent. But in addition to this aspect of the field concept, the further tacit implication of physical reality is almost always present . . . (**6**, pp. 54–58).

It is curious that psychologists have shown so little interest in the *psychology* of human readiness to make these surplus constructions.[10] Like other scientists interested in current criticisms of knowledge, they have been chiefly concerned with the logical and methodological aspects of the problem. And yet, in the historical sequences outlined

[10] That there is a genuine psychological problem here has been pointed out by Holt, who says, ". . . I referred to 'the reifying power of words.' That *names* a psychological phenomenon which may be said to underlie the logical fallacy which we are now considering. . . . But if I supposed myself to have really explained the fallacy by adducing the 'reifying power of words,' I should be committing that very fallacy. . . . why words tend to be taken for things is what would still need to be explained" (**25**, p. 6).

by Skinner and Bridgman, there is an order of events of considerable psychological importance. In both cases, the concept, as a construct pared down to a bare correlation, is attained only in the face of successive resistances—resistances referred to as 'natural' and 'remarkably insistent' and as 'the uncritically naive reaction.' It is this order of events—or rather, the indicated pattern of resistances—that is relevant to the present hypothesis; for it serves as a large scale illustration of the claim that human beings in their conceptual reactions are likely to keep as close as possible to the reaction of perceiving objects; that in successive reconstructions of cognitive material they depart very slowly from the pattern of the full perceptual response; and that as they do so, they meet resistances from more habitual and perhaps more deeply rooted modes of reaction, eliminating only gradually the responses that are not essential, and not always eliminating them completely. Thus concepts, as logical constructs in general use, are likely to have characteristics which may be interpreted as signs that the psychological reactions by which they were attained are closely akin to those at work in perceiving objects. Not only is their function that of serving as units for manipulation, now symbolic manipulation, but they are likely to contain logically superfluous references—perhaps hidden references—to features or aspects of objects or situations as empirically perceived; as if human beings, as they moved from more perceptual to more conceptual reactions, did not disengage themselves cleanly from the perceptual mode of response—as if their conceptual operations involved perceptual determinants of which they remained unaware. Such sequences of events and such characteristics of concepts indicate what is meant by saying that the attainment of concepts is psychologically a modification of the perception of objects, and that it is likely to conform to the pattern of the perception of objects as closely as the conditions of the reaction permit.

VI. Perception, Conception, and Cognition in General

III. The third main point in the hypothesis is that all modes of human cognition may be ordered with respect to the perception of objects and the attainment of concepts. This large statement suggests a possibility which will not be worked out at present. From time to time in the foregoing account, hints have been given as to the direction such an enterprise would follow. Some perceptual reactions, for example, have been treated as pre-typical as compared with the response of perceiving a concrete, manipulable object; remembering and imagining have been treated as modifications of the typical form in the opposite direction, some varieties keeping very close to the perception of objects, others involving elaborate constructions; and the concepts used illustratively have included readily attained constructs, like 'red' and 'apple,' and others which, like the concepts of the reflex and the electric field, presuppose extensive investigation and elaboration. This treatment illustrates schematically the proposed arrangement of cognitive reactions with reference to a line determined by two kinds of cognitive performances: one selected as typical and the other, characterized as distinctive, regarded as itself a modification of the typical form.[11]

But merely to order cognitive reactions with reference to such a line, as more or less typical, as more or less like

[11] One promising line of investigation is suggested by Ryan (49) in his discussion of intermediate objects—intermediate between apprehended and comprehended products in the context in which he uses the term, but interpretable in the present context as intermediate between concrete objects having full 'thing character' and abstract concepts.

the perception of objects, would not take into account the claim that the perception of objects is not merely typical but dominant. It would still be necessary to show what is meant by the pattern of dynamic relationships expressed in the statement that the distinctive mode of cognition in human beings is not that which is most likely to get into action; that of the two kinds of reaction selected as points of reference, one is a readier mode of reaction than the other.

Perhaps this is the place at which to return to the experiments, mentioned in the introductory remarks as those which, as a matter of fact, suggested the hypothesis.[12] For in these experiments, just this pattern of dynamic relationships was indicated. Some of them may therefore be used illustratively. Those which best serve this purpose were ostensibly experiments on memorizing,[13] in which it was the subject's task to learn to identify by name—*i.e.*, by designated nonsense syllables—drawings presented to him successively, one at a time, in sixteen successive series. A series consisted of nine different drawings and no drawing was used in more than one series. Each series was presented mechanically at a uniform rate and the nonsense syllables were so distributed throughout the different series that it was possible, but not necessary, for the subject to discover, as series after series was presented, that the same name was always applied to drawings of a given sort—always to drawings of a tree, for example, though no two pictured trees were alike; always to drawings of something circular—whether of a pictured object or a circular figure or design; or always to drawings of collections of items of a given number, say five,—to drawings of five objects, of five geometrical forms or of five nonsense figures.

Sooner or later, on the first presentation of a series, the subject, without waiting to be prompted, named a drawing on its first appearance—and named it correctly. When he reacted in this way to all drawings serving as instances of a given concept throughout the experiment, identifying each instance on its first appearance—*i.e.*, correctly naming a drawing he had never before seen—he was considered to have attained the relevant concept, and the series in which this consistent behavior began was taken as the point in the experimental procedure at which an adequate concept was attained. In each experiment it was possible to attain nine concepts—three referring to concrete objects, three to spatial forms, and three to numbers. About 200 subjects, studied individually, participated in this set of experiments, different numbers of subjects, varying from about 20 to about 60 working in different experiments. At present, only the trend of the evidence will be indicated, but the statistical data on which the following account is based are available to anyone who is interested.

The most conspicuous fact brought out by the experiments was that in these conditions concepts were attained in a regular order—concepts of concrete objects first, of spatial forms next, and of numbers last. This order was reliable in that concepts belonging to different categories were regularly separated from each other by statistically significant differences between measures of the

[12] This paper is the first publication of any length concerning these experiments. From time to time, informal reports on data from various parts of the research have been presented orally—at meetings of the A. P. A. (22, 23, 24) and at meetings of graduate seminars at Harvard, Yale, Brown, Clark, Stanford, and California. The present paper, it is hoped, will provide the theoretical setting for and thus serve as a general introduction to forthcoming reports of the detailed results of the separate experiments.

[13] The procedure used in these experiments is a modified form of that used by Hull and described in detail in his monograph (26).

points at which they were attained. The order was remarkably constant from person to person as regards the three categories—object, form, and number—but within a single category there were variations in order. The order was constant, too, from one set of conditions to another. For example, in one experiment, conditions were varied by using 12 concepts and 6 names, the names assigned so that instances of two concepts—usually of concepts belonging to different categories—were called by the same name. In other words the subject had to attach two meanings to each of 6 names, in conditions in which twelve concepts were being attained at the same time, and in which the two meanings of the same word were in competition with each other. Here, too, the concepts were attained in the regular order—the four referring to objects first, the four referring to numbers last, and the four referring to forms coming between. To put the same fact in another way, the two meanings for the same word were established in a predictable order. If the word denoted two concepts belonging to a single category —say that of spatial form—the two meanings were established at nearly the same time. If the word denoted concepts of different categories—say object and number—the concept referring to the object was attained at a point significantly earlier than that at which the concept of number was attained. It is interesting too that here, as in the other experiments in this research in which drawings were used as experimental materials, the order in which the concepts were attained was independent of the relative familiarity of the concepts. Unfamiliar concepts of spatial forms, the instances of which were presumably novel to the subjects, were regularly attained later than familiar concepts of objects and earlier than familiar concepts of number. Similarly throughout the experiments, the order of attainment proved independent of the possession or lack of conventional names in the English language, of the particular nonsense syllables used as names during the experiment, and of such characteristics as simplicity and goodness of form in the drawings presented.

There is evidence, too, that the order was not correlated with the detailed procedures by which various subjects attained the various concepts. There was an abundance of evidence throughout the experiments of the behavior called trial and error, and there were less frequent but clear-cut indications of the sudden reactions to which the term insight has been applied. But trial reactions and insights followed the regular order—the first trials and the first insights were to objects, the next to forms, and the last to numbers. And, not only were the adequate concepts eventually attained in the order indicated, but in the course of arriving at them, the subjects reacted to aspects of the presented situations in that order. In the first series in the experimental session, where the task was merely that of learning to name the presented drawings, the subjects treated the drawings as representing objects whenever possible. They seemed to learn the nonsense syllables as names of the objects pictured, or of objects they read into the drawings. In subsequent series, when they began to react conceptually, their trial reactions, including their trial insights, followed a regular course. If reactions to a kind of object proved inadequate, reactions to a kind of form followed. If that reaction proved inadequate—as of course it did to instances of concepts of numbers, which were represented by drawings of collections of items—considerable time was spent on the spatial arrangement of the items. Many subjects went through a stage of reacting to these drawings as scattered or grouped in various ways or

as more or less symmetrically arranged. Sometimes, too, they reacted to vague quantitative aspects like more or less, or odd or even, before determining the precise number, five or six. The whole picture they presented is one of adhering as closely as possible to the presented situation taken perceptually, and taken first as representing a concrete object—of withdrawing very gradually, one might almost say reluctantly or as if impeded and held back.

Yet they did withdraw. In the press of circumstances they went on, responding to successively less thing-like aspects of the drawings until they had produced reactions to just those features or aspects that constituted the common and distinguishing characteristic of a class of instances. This is a point which must be set side by side with the dominance of the reaction to concrete objects; and the persistence of this mode of reaction is the more impressive because, so far as instructions and experimental procedure were concerned, the subjects might have adequately met the requirement of the experiment by merely memorizing the names of each series of drawings as a separate and independent task. Yet as soon as they began to respond to differences and similarities—and many of them did so in the second or third series—they began to react to one after another selected characteristic that might convert separate drawings, scattered at random throughout the experiment, into instances of a class. And this procedure was continued until an adequate concept had been attained or until the experimental session ended. The whole course of events was such as to recall a comment by Freud, who has never been accused of overstating the role of intellectual activities. "We may insist as much as we like," Freud said, "that the human intellect is weak. . . . But nevertheless there is something peculiar about this weakness. The voice of the intellect is a soft one, but it does not rest until it has gained a hearing. Ultimately, after endlessly repeated rebuffs, it succeeds" (14, p. 93).

Here in the successive reactions of individual subjects, in the course of single experimental sessions, there occurs an orderly sequence of events similar in direction to that indicated in the historical development of the concepts of the reflex and the electric field. Again the readiest conceptual response is one that keeps close to concrete objects and every step toward a more abstract construction seems to be made against resistance. Nevertheless reaction to the more abstract does occur; and successive responses are made to increasingly abstract aspects of the situation until eventually the adequate reaction is established as the regular mode of response. This is the dynamic pattern which, according to the present hypothesis, can be found among the cognitive activities themselves: on the one hand a dominant mode of reaction, readily aroused, getting into action vigorously, and especially adapted to the control of motor manipulation; and on the other, a less insistent but very persistent mode, separating itself off only gradually from the dominant mode, involving responsiveness to more and more abstract aspects of a situation, and especially adapted to operations with symbols.

VII. Relation to Other Points of View

It would be strange if anyone listening to the foregoing account had not frequently been reminded of Goldstein's (16, 17) distinction between concrete and abstract behavior. Clearly the treatment shows marked similarities to Goldstein's—similarities, that is, insofar as it concerns behavior. An important part of Goldstein's contribution is his physiological theory, and the hypothesis

here presented is physiologically neutral. It is evident, too, that there is not complete agreement in the definitions of concrete and abstract behavior. For example, Goldstein works with a dichotomy and the present paper emphasizes a continuity of reaction between the two; Goldstein keeps to a broad classification and the present hypothesis works from two specific modes of reaction singled out from the two general levels of behavior which Goldstein calls concrete and abstract. Yet both approaches seem to lead toward the same kinds of facts, and from both points of view some of the same kinds of relationships appear. Above all, in both accounts cognition is treated as dynamic. A pervasive and impressive feature of Goldstein's work is his interpretation of cognition in its many forms as a way in which an impaired human organism reorganizes his environment in the very process of apprehending it, establishing a *milieu* suitable to his own capacities for reaction. Thus Goldstein not only points to dynamic relationships within the field of cognition itself, but treats them as parts of a larger system of relationships in which cognitive processes figure dynamically in the behavior of the organism as a whole.

The present hypothesis also resembles a mode of treatment implicit in much of Woodworth's writing. Woodworth has long interpreted the behavior of living organisms as reactions to objects in a field of objects. From time to time he has stated his thesis briefly (*e.g.*, 58) but he has not yet developed it explicitly and at length. Significantly, however, he warns the reader in *Experimental psychology* (59) against a probable theoretical bias, especially in the chapters on conditioned response, maze learning, perception of color, and problem solving, all of which deal with the psychology of cognition. There is some similarity, too, between the present hypothesis and Brunswik's view. In one of his papers, Brunswik says: "In a merely functional sense, . . . intuitive perception seems to be a somewhat autonomous but more primitive function (or subpersonality) working in principally the same 'constructive' . . . way as the critical instances of verbalized measurement and computation do. The difference seems to be merely one of degree" (8, p. 257). No two of these points of view exactly coincide, but from all of them the same kinds of facts and relationships are being investigated. I should like to believe that all of these enterprises are approaches to a common understanding.

There are many other treatments of the topic, both experimental and theoretical, with respect to which the present hypothesis should be stated. Above all, objections arising from other points of view should be given thorough consideration. But this presentation must be content with being a *mere* presentation. A hypothesis has been stated and illustrated; it remains a hypothesis and nothing more.

VIII. Relation to Research

There is one point, however, which cannot be overlooked in presenting any scientific hypothesis, even in a preliminary statement. This is its relation to experiment and research, a point of special importance in a broad molar treatment like that just given. Many psychologists are especially alert to the dangers of such over-all views—more alert to such dangers than to those to which they have become habituated. At any rate, there is a belief, despite the increasing recognition of molar units in psychology, that wide relationships are especially difficult to pin down to the solid ground of fact. It is necessary, therefore, to emphasize the point that the hypothesis as formulated is directed toward research. The key psychologi-

cal processes, it will be remembered, have been defined in terms of the organism's behavior as related in specified ways to objects in the external world and to interpersonal concepts. It has been pointed out that in actual practice, the investigation of such processes consists in determining the conditions in which an organism produces a kind of reaction selected by the experimenter as significant—in which an organism performs what for brevity is often called the experimental 'task.' And the investigation of performance in that sense of the term is one of the fields in which psychology is most at home and most successful. It is, in fact, a method that has characterized much psychological research since it was first employed in the first psychophysical experiments. It has since been used in investigations of sensory and perceptual processes; of learning, motivation, and problem solving; in the measurement of intelligence and special abilities. Essentially it is objective. Essentially it consists in determining the extent to which an organism produces a specified kind of behavior in conditions determined as precisely as possible. It is a method which enables an experimenter, by suitably varying conditions, to determine more and more exactly the conditions in which a specified reaction occurs, thus closing in step by step on the solution of his problem. The special problem put by the present hypothesis is that of determining for specified performances, for which specified concepts are essential determinants, to what degree and in what direction they involve departure from the response of perceiving concrete objects, and in what conditions and to what degree such performances occur. The point that here requires special emphasis is that it is as possible to control conditions with respect to concepts as with respect to material objects. It would be folly to enter upon such a program with any expectation of rapid progress, or in any way to minimize the difficulties and complexities involved. But it would be folly quite as great to refuse the attempt. The hypothesis indeed points to broad relationships, but it places them in a context in which they can be investigated.

IX. COGNITION AS DYNAMIC

As a matter of fact, the hypothesis has ramifications wider than those mentioned. Cognitive reactions have been considered so far chiefly in relation to the organism's motor and symbolic behavior. But a moment's reflection shows that they are thus brought into relation with the desires, drives, and emotions, so intimately involved in determining the functional significance of the objects and situations reacted to and referred to—with the whole array of non-cognitive determinants of action sometimes considered the special subject matter of dynamic psychology. For cognition is here treated as the very means by which the environment is molded to suit the needs and reactive capacities of the organism—as the means by which the organism is presented with objects desired or shunned, with a theater of reaction which makes their attainment or avoidance practicable to its modes of operation, with tools which enable it to work indirectly as well as directly toward the satisfaction of its needs. So regarded, cognition has its specialized function as do other activities of the organism, and coöperates with non-cognitive processes in determining behavior. It has been customary in psychology for the last two or three decades to emphasize the non-cognitive determinants of behavior, and indeed the non-cognitive determinants of the cognitive processes themselves. This emphasis was sorely needed and the facts to which it called attention must be recognized in their full impressiveness.

But they must not be permitted to obscure what is also a fact—that cognitive processes have their special and positive function in the adaptation of the organism to an environment which it cannot completely transform to suit its own preferences; that in performing their function cognitive processes are capable of considerable resistance to non-cognitive demands; that there are limits beyond which perceptual and even intellectual processes cannot be pushed if the organism is to survive in the world and the society in which it finds itself. A dynamic psychology would give an incomplete and distorted account of human behavior if it ignored or minimized the problems arising from this situation.[14]

It is curious that the term 'dynamic psychology' is often used today to denote a field with which the psychology of cognition is contrasted. The historical reasons for the practice are too obvious to require comment. But historical events, however illuminating concerning the behavior of psychologists, do not determine the logical structure of psychology. Fortunately the results of historical accidents are not always irreparable. It is neither impossible nor unreasonable to define dynamic psychology as including *all* the psychologically significant determinants of behavior. If this definition were accepted, dynamic psychology would take as its subject matter, not a selected class of psychological processes, but all psychological processes considered from the standpoint of its special problems. The hypothesis here presented assigns to the cognitive processes a place *within* the field of dynamic psychology, so defined. And the pattern of dynamic relationships it traces among the cognitive processes themselves, takes on an enlarged significance when these processes are regarded as engaging with other activities of the organism as effective determinants of its behavior.

In closing I should like to emphasize two words contained in the title of this paper. On is the term *dynamic,* in the sense just indicated. The other is the word that expresses both incompleteness and hope—the groping preposition *toward.*

[14] It is interesting that when, within the field of dynamic psychology itself, attention to what might be called its conventional subject matter has become less exclusive, the dynamics of the personality have been reinterpreted. The most striking single illustration is that provided by Freud (12, 13, 15) in his study of the ego trends and his reorganized interpretation of the personality as a whole. Other illustrations are provided by Murray's (45) attention to a manifest as well as to a latent personality, and Allport's (1) pervasive insistence that the individual's 'mature' interests and abilities be taken into account in a study of personality.

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THE OBSOLETE DOGMAS OF HEREDITY

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I. Introduction

None of the issues of psychology is of greater theoretical or practical importance, and none is more confused than that of the relationship between heredity and environment. Many writers of textbooks approach the problem with manifest trepidation, and discussions of no other topic in psychology are quite so clouded by an irresolute eclecticism. Students and teachers alike seem to have learned that when questions of inheritance rise, equivocation is always the best policy.

Yet clarity of thinking about nature-nurture is vitally necessary because our interpretation determines for us the practical possibilities and limitations of manipulating either one or the other, or of the two concurrently, in order to improve human living. Any plan of human betterment, which, owing to misconception of natural realities, on the one hand aims at goals which are biologically impossible, or else fails to utilize available possibilities for improvement on the other, is doomed to eventual disaster, regardless of the enthusiasm of its present advocates. A major difference between Nazi, Soviet, and American social programs arises from differences in conception of the functions of heredity and environment. The relative survival-success of these cultures will be a measure of how clearly they reflect actual reality.

It was formerly a favorite device of writers of textbooks on psychology, especially social psychology, to make the nature-nurture controversy the principal *pièce de résistance* of the book, so that in effect it constituted a prolonged debate on instinct. The argument continued from one supposedly-unique, mental function, such as sympathy or imitation, to others often only nominally different, such as coöperation or suggestibility. Even some of our most recent texts exhibit much the same methodology, except that the old issue appears in a new guise; for instance, 'tissue needs' *vs.* 'social needs,' or 'dependable motives' *vs.* 'cultural motives,' or perhaps the ego *vs.* the super-ego, but usually the older nativistic bias is now replaced by the contemporary ideological partiality for environmentalism.

Writers of other modern textbooks have largely avoided the noxious controversy, possibly because they can see no resolution of it and discussion must therefore be indeterminate and fruitless. It is fairly obvious, however, even to the brighter elementary students, that there is some sort of evasion, and that such texts are incomplete because certain natural and inevitable questions about life are left unanswered. But they usually learn sooner or later that 'nice' students never ask certain embarrassing biological questions.

The nature-nurture dilemma in which modern psychology is entangled is not so much the consequence of a shortage in relevant evidence as of a certain failure to assimilate and utilize currently available facts about heredity. It is true that there is great need for further experimental work to examine certain unexplored, behavioral aspects of the genetic problem, but the conception and execution of such experiments necessarily await clarification and reorientation of the issue in the minds of experimental psychologists. Theoretical

reviews of the status of the genetic problem, biological and psychological, have not been very effective in achieving a popular reconstruction of the traditional ideology. In spite of the contrary evidence of recent biological research, we still cling to many of the genetic concepts of the last century. The social psychologist, particularly, has failed to achieve a transfer from the more generalized formulations of biology to their specific applications in psychological textbooks; which, presumably at least, should have the simplicity and clarity of statements in objective examinations.

It occurred to the writer that it might be worth while for some psychologist to attempt to reduce the issues of nature-nurture to the definite, decisive form of true-false test items, and to publish these in the hope of stimulating other psychologists to reëxamine their own attitudes toward the heredity problem in order to decide about the truth or falsity of the assertions. The writer presents herewith such a list of statements, some of which were taken from contemporary textbooks. Incidentally, he also takes his own examination, that is, marks each statement as *true* or *false* according to his opinion, and also attempts a justification of his judgments. This does not mean that the writer nominates himself as a biological authority or arbiter of genetic issues. The purpose is simply to reduce the problems of psychological inheritance to precise, definitive form in the hope of furthering discussion.

II. TRUE OR FALSE STATEMENTS ABOUT HEREDITY

1. *There are instincts. Parental care of offspring is an instinct.*

FALSE

The minimal and irreducible meaning of the word *instinct*, as long established by common usage and general agreement of definitions of psychologists, is the development of a certain kind of complex behavior without the necessity of practice or learning, in other words, a behavioral development determined entirely by heredity. Detailed study of many behaviors, formerly accepted as instinctive, has often shown, however, that learning does play a necessary part in their development. It has really not been demonstrated that learning is absent in any motor development, even of the simple reflexes. The reasonable presumption, therefore, is that learning plays a necessary part in all behavioral growth. Consequently, if the long-established definition of instinct is accepted, there are no instincts. The instinct hypothesis is impossible because it sets up an impossible test of instinct; namely, that development shall come wholly from within, without external assistance.

2. *Innate tendencies coöperate with environmental influences to organize behavior; parental care is neither exclusively inherited nor entirely learned.*

TRUE

The conclusion that there are no instincts does not demand acceptance as a corollary that heredity is without formative influence on behavior, that parental care, for instance, is entirely learned. Such a claim is manifestly absurd. Because of heredity, it is probably easier for a human father to learn to care for his offspring than for a tom cat to learn to care for his. In so far as men and tom cats are concerned, there is no experimental evidence which either directly proves or disproves this claim, but, in its absence, general observation seems to warrant the opinion that innate factors do influence some creatures to learn some behaviors easier than other creatures, and likewise, also, to learn some behaviors easier than other behaviors. It is entirely possible that,

with the same training from birth, some men would learn to be better parents than others.

In an article published in 1934, 'Heredity as a differential element in behavior' (5), the writer contended that differences only are hereditary or environmental, and proposed that comparative *ease of learning* be substituted for *development without learning* as a criterion of hereditary influence. If with equal opportunity to learn two different responses, one is learned more readily than another, this difference in learning must be owing to previously-developed inner momenta, commonly called heredity. No absolute distinction between heredity and environment, as entities extending either before or after birth, is either practicable or possible. Learning plays a part in all development, and earlier development always influences later learning. Solution of the practical problem of evaluating 'hereditary' influences is necessarily dependent upon and relative to selection of some arbitrary boundary between an earlier or causative period of development and a later development which it influences. In order to be able to compare facility in learning different behaviors, it is necessary first of all to agree when these periods are to start and stop. Depending on circumstances, any convenient line may be chosen, but scientific expediency seems to demand a commonly-recognized one.

It is implicit, although not explicit, in our common-sense understanding of hereditary that its meaning is approximately the same as *innate* or *inborn*. This paper urged that, provided all traditional connotations of discrete genetic sources are disavowed, this implicit identity of meaning should be openly avowed and used. It recognized that whatever the baby is at birth is a mixed resultant of many unidentifiable internal and external influences, but asked in effect that we agree to let bygones be bygones, that the slate be wiped clean, and that whatever the baby is at birth, in so far as its influence on his future is concerned, be known as *heredity*. Acceptance of this usage is in no way a denial that there are chromosomes, that there is a prenatal development resulting from the interaction of many factors. The paper does claim, however, that no combination of these factors has a continuously separate existence or is permanently identifiable, either as heredity or anything else. Therefore, by common agreement, the meaning of *heredity* should be the *innate differential in development*.

In an article by Donald O. Vine and the writer (6), published in 1940, the influence of heredity, as defined above, on subsequent learning was reliably ascertained. The premise on which this experiment was based was not the usual one; the evidence of heredity was expected to show, *not in the absence of learning, but rather in and through learning*, by differentiating it. Equal numbers of bantam and leghorn chicks were hatched together, reared together, and each chick given exactly the same training to go to his own kind of chick for food as to go to a chick of the other kind. In other words, heredity was the only thing that could make any difference in learning to go to a chick of own kind rather than to a chick of the other kind. Learning to go to own kind was reliably faster and easier, a fact which demonstrated, as claimed above, that with the same training from birth some creatures learn some behaviors easier than other creatures. In this experiment, moreover, this result was not obtained because one kind of chick was generally more intelligent than the other kind, since *all* of the chicks, *both* kinds of chicks, learned to go to their own kind more easily. The justified inference, therefore, is that because of innate

differences, other than in intelligence, one man may learn parental care more easily than another man, or learn aggressiveness more easily, or acquisitiveness, or coöperativeness, or homosexuality, or the incest taboo, or any other behavior.

3. *Some characteristics are inherited while others are produced by environment; a dog's bark is inherited but his response to his name is acquired; blue eyes are inherited but a coat of tan is environmental; sex is determined in the chromosomes but fashions in dress are cultural.*

FALSE

All characteristics, physical and behavioral, human and animal, are joint products of heredity and environment, and would not appear if the contribution of either was left out.

Does this mean that there is no real difference in the characteristics apposed above? Certainly there is a difference, but it is not that some characteristics are inherited while others are acquired from the environment. Since development is a joint product of all inner and outer factors, and not the exclusive product of any portion of them, it follows that changes in any part of them may change the total resultant, but cannot determine it. The actual difference in the contrasted traits is that the *differences* most frequently observed between individuals in the so-called hereditary traits (not the traits themselves) result from variations in breed or lineage, while observed *differences* in supposedly environmental traits result from variations in the environment. "Dogs delight to bark and bite, but little children never," so barking is presumably inherited. Domesticated dogs respond to a name but wild dogs do not, so the name response is said to be acquired. Variations in eye color correspond principally with parentage. As far as most of us know, eyes are blue regardless of changes in environment. Only the skin exposed to sunshine is tanned, so tan is classed as environmental.

Is this mere quibbling about terminology? The reason it is not is that in proportion as our scientific knowledge of genetic factors increases, these dogmatic assumptions that changes in environment cannot change 'inherited' characteristics, and that heredity cannot influence 'environmental' traits, turn out to be wrong more and more frequently. Moreover, if we persistently cling to these obsolete dogmas, research is progressively handicapped, and genuine social progress is retarded.

On the one hand, variations in so-called hereditary traits as a result of variations in environment are numerous. Dogs reared in isolation often fail to learn to bark. Eye color is sometimes changed permanently in childhood by the presence or absence of certain minerals or vitamins. Sex was formerly accepted as unquestionably determined by certain chromosomes in the germ plasm. Yet, certain experiments by R. R. Humphrey (7), L. V. Domm (3), and others, have demonstrated that sex characteristics, morphological and behavioral, can be removed or even changed to those of the opposite sex by administering certain drugs or hormones. C. L. Foote (4) has shifted the sex of amphibia either to all males or all females. F. A. Crew (2) reports a hen which changed into a rooster as a result of glandular trauma, and was both the mother and father of offspring. C. R. Stockard (10) radically changed the bodily structure of minnows by rearing them in water to which magnesium salts had been added; the change was not destructive merely but adaptive reorganization of the whole organism was achieved. The different bodily structures and behaviors

of workers, soldiers, queens, etc., can be produced at will in certain insects by variations in diet or temperature.

Organic diseases and malnutrition often produce striking anomalies in bodily structure, mentality, and motives of humans; they are often corrected by surgery or supplementary diets. Intelligence is supposedly hereditary, yet it is sometimes changed radically by doses of thyroxin, and, to an extent, also by rearing children in a more stimulating environment. Diet was probably responsible to a large extent for certain bodily and mental abnormalities of medieval peasants, and there is reason to suspect that the present peculiarities of appearance and conduct of certain supposed races of men are fostered by their unique diets and by their mode of life.

Variations in heredity, on the other hand, may exert just as much influence on the so-called environmental traits. This is a verified fact of the reported experiment (6) in which chicks learned to go to their own kind more easily than to a chick of the other kind. It is apparently very difficult to train coyotes, a breed of wild dog, to respond to a name. While tan is developed in sunshine, some individuals and races do not tan as easily as others. Except for an inherent potentiality for reacting to sunshine, no one could acquire a tan. Lack of hairy protection from birth undoubtedly influences men to wear clothing, but while there are individual differences in the actual clothing needs of men, fashion is perhaps the chief differentiator of dress.

The necessary conclusion is that variations in either heredity or environment differentiate the development of all characteristics. No question should rise in the mind of the reader as to which factor comes first or is most important, any more than whether fuel or air is more important in the production of heat. The really significant fact is that prenatal and post-natal influences are mutually essential in the production of every characteristic of every living creature. Even a new-born babe does not nurse well until it has practiced a bit.

The so-called hereditary traits can only be discovered by comparison of two or more individuals reared in similar environments. If only one individual were available for observation, it would be utterly impossible to decide which of his characteristics, physical or behavioral, are 'hereditary' or 'environmental.' No characteristic is ever solely, or even chiefly, the product of either heredity or environment.

Shepard and Breed (9) found that chicks, fed artificially from time of hatching and not allowed to peck at food particles for a few days, were far less efficient in pecking than normally-reared chicks of the same age. The fact that, when the retarded chicks were allowed to practice pecking, they improved very rapidly, much more rapidly than new-born chicks, proves that practice is essential for this development, but it does not prove that practice was the sole or major cause of the development. Likewise, while the accelerated learning of the older, retarded chicks demonstrated the contribution of maturation to development, it does not prove that maturation *is* development, or is the sole cause of development. Rather the experiment proves the mutual dependence of both factors. Practice makes a difference, and heredity makes a difference, but neither has primacy or a monopoly in any growth.

The real difference between 'hereditary' and 'environmental' characteristics, therefore, is that conspicuous variations in 'hereditary' characteristics, from one individual to another, are always or usually associated with earlier germinal or innate differences, while the commonly-noticed variations in so-called environmental characteristics generally

follow noticeable changes in the environment. These differences are not in the number or importance of hereditary or environmental components, but in the effects of *variations* of either of these in different individuals, as observed by an interested observer. When hereditary variations in a trait are most conspicuous to the observer, he usually ignores the environmental contribution and says that the trait is hereditary; if, on the other hand, environmental variations are most evident, he is tempted to designate the trait as environmental, failing to recognize the indispensable contributions of a common heredity.

If these linguistic miscarriages were only scientific shorthand, our present emphasis would be unnecessary. But they are not. Because language was vaguely abbreviated in times past, the actual semantic referents of the words *hereditary* and *environmental* are now not recognized, and a serious miscarriage of meaning and consequent social action is the present forfeit.

In this semantic reference, it is important to recognize that 'conspicuous' differences are always conspicuous to someone. The nature of the observer is just as important as the phenomena observed in determining his observations. In some instances, perhaps, innate factors really are responsible for the observed variations; in others, they may actually be owing principally to variations in environment; in still others, the observed difference may be largely an artifact of selective attention, mental bias, or sensory limitations of the observer. Usually only the more superficial aspects of behavior are actually visible. Irrespective of perceptual limitations or distortions, however, it is still true that in no instance does either heredity or environment have either a monopoly or a majority vote in determining any physical or mental characteristic. To repeat: *Differences only are hereditary or environmental; characteristics of any kind, either physical or mental, are not.*

Every trait is actually a composite resultant of a developing situation, which is in turn a part of, and controlled by, a larger or total development. The slightest change in any of the factors involved is bound to change the resultant, but that should not beguile psychologists into assuming that the changing factor is solely responsible for the resultant. A multitude of other differential factors, internal and external, are always there, silent partners in every development. The effect of variation in any of them is obtained only through coöperation of all others.

4. *There are innately-differentiated and environmentally-differentiated characteristics, but not hereditary or environmental characteristics.*

TRUE

5. *Some characteristics are the result of maturation while others are the result of use; ability to nurse or walk results from maturation, whereas skill in skating is achieved by practice.*

FALSE

The word maturation is usually limited in meaning to the physical aspect of hereditary development; therefore, this statement refers merely to the organic aspect of the previous one, and the same evidence, especially that relating to variations in physical characteristics, such as eye color and bodily form, which result from variations in environment such as in diet, temperature, or chemical environment, justifies marking it *false.*

Many writers have mistakenly assumed that structural growth of the organs and form of the body, and also the functions made possible by that growth, are quite obviously determined by he-

redity, and that while environment may supply the materials of growth, it has no formative influence on the pattern of growth. As was said previously, this misunderstanding arises from a common tendency to assume erroneously that the factors which vary most, and produce the most conspicuous differences in development, are the only developmental factors, or at least, the most important factors. It also reflects the old, disproved preformation theory, that every bodily characteristic has its antecedent in the germ plasm. It is true, of course, that the most frequent cause of *differences* between individuals in the grosser traits, such as in size, strength, appearance, and physical skills, is obviously heredity. Since the environmental contribution to all the typical or normal developments, such as arms and legs, or walking, is generally very much the same for everybody, its importance is commonly overlooked. And as is often the case in politics, and sometimes also even in scientific research, it is the variable, conspicuous contribution which gets the most recognition.

Even though the only differences in growth which are sufficiently outstanding to be noticed arise from differences in parentage, there is every reason to believe that the unrecognized contributions of other factors are just as important. In certain rarer instances, particularly experimental, in which it is possible to observe differences in 'matured' form or behavior which result from variations in environment, the developments are sometimes sensational. Examples are the one-eyed fish reared by C. R. Stockard (11) in water containing magnesium salts, or the stupid and deformed persons suffering from hypothyroid owing to a diet deficiency in iodine.

There is no logical reason for believing that heredity plays a larger part in 'maturation' than in 'development by use,' or 'learning.' 'Matured' traits are joint products of inner and outer factors as are all traits, but *conspicuous* differences between individuals in 'matured' traits are manifestly not the result of training or external factors; but rather arise from inner factors, from originally different germ plasms at different stages of development.

'Maturation' is simply development in which commonly observed differences between individuals are correlated with previous differences in the inner organism rather than in the environment. Externally-induced variations might be just as conspicuous if the effective environmental combinations were only known. Once this fact is recognized, we may dare to try unusual combinations of external conditions which may very well produce strange shapes and behaviors such as the world has never seen before. To some extent this result has already been achieved in floriculture; new colors and patterns of blooms are commercially produced by variations in temperature, moisture, and soil content in which the plants are grown. New comb and feather formations of fowls exhibited in recent poultry shows were produced by injections of various hormones or enzymes.

It is true that the results of certain recent experiments have been interpreted as favorable to the maturation theory. They show typically that development has occurred in spite of the fact that supposedly-essential training or practice has been prevented. Stone (11), for instance, reared male rats in isolation until they were sexually mature, whereupon they were placed with sexually experienced and receptive females. Copulation was achieved so quickly that it was concluded that learning could not have occurred.

A first reflection on this experiment is that relatively similar results prevail in the development of all behavior, especially sexual behavior. Boys and girls,

reared practically in isolation, at least in so far as sexual experience is concerned, if brought in contact when the sex drive is strongest, are often themselves greatly surprised that insemination is accomplished so quickly. Yet in such instances it would be incorrect to say that there is no learning, even if the participants did happen to miss the benefits of sex education, either formal or informal. The gist of the matter seems to be that such learning is greatly accelerated by inherent factors, that there is an unstable total situation in which things and events rapidly fall together, as in a reversed movie, or in the formation of crystals.

There is probably an admixture of such tendency in all learning, even in philosophical speculation or in deriving relativity equations, and it is very improbable that learning, *i.e.*, the time element, is ever crowded out completely. It takes time for calves, or even wasps, to learn to walk, in spite of the fact that they learn more quickly than humans. Stone admits a brief period of hesitation even in the case of the rats. Other segregated and probably older animals, such as cats reared indoors, are very slow in learning to copulate. If one thinks in terms of the total situation in Stone's experiment, rather than of the males alone, there is manifestly the ready and also experienced female to facilitate achievement.

Because the prevalent concept of practice is unreal, it is impossible either to prove or disprove that maturation really happens. When claims are made that maturation has occurred in certain instances, proof always depends upon the validity of evidence that practice has really been with-held. In order to establish such validity, it is necessary to decide exactly what is practice. It is tacitly assumed in such claims that practice is previous performance of the act, that is, the *exact* act as finally observed after 'maturation' was finished. In case of the rats, previous copulation presumably would be such practice. Obviously, however, learning by practice is really never simple repetition, over and over again, of identically the same activity, as is commonly assumed. If this were true, certainly, there could be no learning because progress is ruled out by definition. If later 'practices' are different, either spatially or temporally, they are certainly not repetitions of earlier activities. Purported practices, involving so-called 'repetitions,' are really new emergences because they have never occurred in the same form previously.

The similarity of acts in a learning series, as represented by a learning curve, is really superficial; it is inferred from the recurrence of some easily-identified development which, according to the opinion or prejudice of an observer, is supposedly a consummation. The actual pattern of movements leading to this culmination always varies progressively. The organization of movements by which a person first catches a ball, for instance, is very different from that in the later catches. The improvement in organization is undoubtedly a cause of increase in the number of catches, but successive 'catches' are not mere repetitions of previous ones. If it is recognized that progressive change, or 'improvement,' in activity always accompanies that which we call practice, then it must be clear that the later actions cannot be identical with the earlier ones.

If the above reasoning is correct, then every act is a maturation, because, if previous actions are different, they certainly cannot be called 'practices,' since, by definition, practice is repetition of the same act and maturation is development without practice. If it is claimed, by way of refutation, that the successive acts need not be identical, but only simi-

lar, then the lid is off entirely, because the next questions are, how similar? and who and what decides whether they are similar? There would be no way of determining where to draw the line, of deciding how different the previous activities might be and still be accepted as practices. If an animal is now alive, there certainly must have been a long regression of previous, self-preservative activities. The earlier antecedent acts were probably so very different that, superficially at least, they might seem to have nothing in common with the later ones.

In the case of Stone's rats the activity program which was presumably identified by penetration of the vulva, evidently had a long and varied genetic lineage of previous actions, some overt or explicit, and some covert. While this program had not previously involved other rats, it evidently had involved being handled by the experimenter, and also possible masturbation against the cage, food troughs, etc. Previously also there had been contact with the mother and siblings. In the broader and only real sense, there had evidently been a great deal of preliminary practice.

The same uncertainty prevails in regard to the well-known experiment by Carmichael (1), in which apparently-normal frogs grew from eggs developed in an anesthetic solution. There is no clear evidence either that preliminary activity was altogether prevented, or that the behavior finally developed was altogether normal. The inner bodily activities were maintained in the anesthetic, obviously, else the frogs would have died. Also, there was a short period of rapid improvement in activity after removal from the anesthetic which, while faster than "pure" learning, was nevertheless not instantaneous. Judgments of the normality of activities in this experiment were necessarily estimates, and perhaps not entirely reliable.

The writer believes that a critical survey of all instances of supposed maturation or inheritance would convince anyone that there are no phenomena which cannot be better accounted for as joint products of heredity and environment, with one or the other inducing more striking differences between individuals in some instances, than in terms of the old conception of separate genetic sources and processes. All of the established and operationally-tested facts and laws of genetics should be just as rational, and more usable, if heredity is interpreted as a differentiator, rather than as a determiner of growth.

6. *Physical characteristics are obviously hereditary, or more largely hereditary than behavioral characteristics.*

FALSE

If the previous argument is valid, this interpretation must be abandoned, since it has been shown previously that physical characteristics are sometimes profoundly differentiated by environmental factors, such as use, food, chemical agents, moisture, temperature, altitude, etc.

7. *The pattern of later development of an organism exists in miniature in the germ cell from which it grows.*

FALSE

This is a hangover from the discredited preformation theory of the older biology. It was once assumed that the development of an organism is like the blooming of a flower bud, merely an unfolding of that which had previously existed within. It is not true, even for the bud. There is no reason to believe that the blueprint of later structure is any more to be found in the antecedent germ plasm than in the world at large. Like the shape of rock crystals, the pattern results from interaction of many

factors. Any one of them can differentiate the total pattern, but none is a carrier of it, nor can determine it.

8. *The great variety of actual or phenotypical forms of structure and behavior are best accounted for as environmental 'modifications' of specific antecedent forms, or genotypes, hidden away in the germ plasm.*

FALSE

This is a particularly tricky disguise for the old preformation dogma. W. Johannsen (8), the discoverer of hereditary 'pure lines,' clearly recognized that phenotypes and genotypes could not have been discovered, and should not be conceived, independently of a normal environment, which on the average remains so constant that the innately-differentiated ratios are not significantly shifted by changes in it. He emphasized the great pains he took to rear all his plants in a uniform environment when hereditary variations were observed. Mendel's law also is valid only if this limitation is recognized. Usually and practically, of course, these established hereditary ratios are dependable and usable guides in plant and animal breeding, but blind, unconditional acceptance of them as absolute and self-sufficient laws of heredity may, and often does result in theoretical and practical confusion about what new forms are really possible. The so-called *determiners* within the chromosomes are not really determiners, but only *differentiators*.

9. *While behavior is not instinctively determined, there are inherited 'tendencies' toward certain acts.*

FALSE

This is the obsolete preformation dogma in another popular disguise. 'Tendencies' are just as much a product of the normal environment as of inheritance, and could not exist in the absence of either. Tendency of a bullet to hit a target is as much dependent upon the position of the target as of the gun. This common mode of rationalization of the instinct theory holds that there are latent instincts, 'tending,' as William McDougall says, 'toward a specific form of behavior,' but they are 'modified' into a great variety of final forms by environmental influences. The implicit meaning of the words 'tendency' and 'modification' in this reference cannot be anything else than preformation; since they imply that, except for outside interference, heredity would of itself achieve its normal end. It is tacitly assumed that the direction or form of development is inherited, even in the very act of proposing that it may be perverted or 'modified' by environment. Predetermination of a total resultant by a limited and monopolistic corporation of autocratic factors, however, is something which never happens in nature, at least for very long, either biologically or politically.

10. *There are 'natural' tendencies toward certain kinds of behavior which result from the interaction of a given heredity and a normal environment.*

TRUE

11. *Innate and environmental influences are not mutually-exclusive or competitive, but supplementary. It is not 'nature vs. nurture,' but 'nature and nurture.'*

TRUE

There is a strategy in life by which inner and outer factors work hand in hand. Conditioning influences the baby to love the mother who fondles it, but a set-up favorable to such conditioning is prepared previously in the pleasure areas on the mouth, breasts and buttocks of the babe, and in the necessity that the mother shall stimulate these areas while

nursing the baby, and while the baby looks at her. It is a clever scheme by which inherent factors become effective, not in spite of, but by means of conditioning. Function, structure, and environment are inter-dependent, not independent; leg bones, the earth beneath, and the physical constant *g*, are part of the walking 'instinct.'

The basic problem of social psychology is not 'heredity *vs.* culture,' but to know how each supplements the other, to understand how and to what extent variations in either, or both concurrently, may vary the total social pattern. Hereditary and cultural explanations are not necessarily in conflict with each other but should be interdependent and complementary; cultures grow out of heredity, and heredity is so devised that a given culture is a necessary means of realizing its normal consummations.

12. *'Learning' is solely acquisition from the environment; in pure form the learning process is independent of 'instinct.'*

FALSE

The experiment with the bantam and leghorn chicks (6), indicated that (with the same training from birth, and irrespective of any superiority in general intelligence, or any difference in inherent difficulty of the tasks) the chicks still learned to go to their own kind of chick more easily than to the other kind. Inherent factors facilitated a certain kind of learning. The distinction commonly made between the processes of learning and maturation is artificial, and often misdirects and distorts thinking and research. The previous criticism of maturation is just as pertinent for 'pure learning.'

13. *Intelligence is inherited.*

FALSE

It should be understood that our judgment that this statement is false incurs no obligation either to deny that differences in lineage can differentiate achievement in intelligence tests, or to insist that such achievement is solely a creation of environment. It would be equally false to say that intelligence is acquired, or that it is chiefly acquired, or chiefly inherited. Differences only are inherited or acquired. On the basis of available evidence, however, it does seem apparent that intelligence scores are both innately and environmentally differentiated.

In the experiment (6), previously mentioned, demonstrating innately-differentiated learning in fowls, it also appeared from a re-grouping of data that the group of bantams learned on the average more easily than the leghorns, both to approach and to avoid both own and other kind of chicks, and in spite of the fact that the training or opportunity to learn was the same from the time of hatching. While the experimental set-up was originally devised to exhibit any acceleration, owing to innate factors, of learning the specific skill of going to one or the other kind of chicks, and to do so independently of the influence of general learning ability, yet when the data were re-grouped, they also revealed an innate differentiation between the varieties in ease of general or non-specific learning (intelligence?). This differentiation was independent of any innate difference in tendency toward the equated, specific kinds of learning. The writer is not aware that such a basic difference between natural groups in general learning ability (aside from the effects of (1) cumulative, post-natal biases in training, and from (2) specific, prenatal learning tendencies) has been demonstrated previously.

14. *If equal practice increases differences between individuals in a trait, the trait is hereditary; if equated piano practice increases individual differences*

in performance, the musical talent is inherited.

FALSE

The trait, as such, is not hereditary, but innate diversity is the cause of the *additional difference* between the individuals. Under conditions of equal or maximum practice, musical ability is necessarily a hereditarily-differentiated trait. For identical twins it would necessarily be either undifferentiated or environmentally differentiated. If equal additional practice diminishes differences between individuals, part of the initial difference was caused by unequal practice, but designation of the trait as environmental on the basis of diminished differences with continuing practice is not warranted.

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PHYSIOLOGICAL DIFFERENTIATION OF EMOTIONAL STATES

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During a recent experimental investigation reported elsewhere (3), the writer noticed that fear reactions or adrenalin injections abolished the sound-produced seizure in rats. The behavior pattern exhibited (either during a 'fear' reaction or after an injection of adrenalin) consisted of trembling, twitching, lip-wetting, a frozen inactivity, and occasionally prostration. The question which immediately arose was: in what way does fear (or adrenalin) prevent attacks? If it is done, according to Cannon's emergency theory of emotion, by energizing the organism, by "providing the best possible condition for putting forth supreme muscular effort" (14, p. 196), why did the condition observed resemble extreme muscular weakness rather than muscular strength?

This obvious contradiction led to a review of the evidence for Cannon's contention that during emotion (any emotion, fear as well as rage), the observed secretion of adrenalin puts the organism on an emergency footing and serves as preparation for fight or flight.

Cannon's emergency theory of emotion was originally propounded (14) on the basis of a series of experiments in which he showed that (1) the secretion of adrenalin is increased in fear and anger, (2) adrenalin injection or perfusion restores the size of muscular contractions in a fatigued nerve-muscle preparation. Earlier findings that removal of the adrenals has a debilitating effect on muscular power and that an adrenal gland extract has the power of increasing and prolonging the contractions of normal resting skeletal muscle (Oliver and Schafer, quoted by Cannon, 14), was taken by Cannon as supporting his thesis.

In the meantime, a great deal of research has established that not the medullary hormone (adrenalin) but the cortical hormone (cortin) is instrumental in maintaining muscular strength (13, 23, 33, 37, 38, etc.). Thus the fatal results of adrenalectomy have been traced to loss of the cortical, not the medullary, function. As Oliver and Schafer's extract contained both adrenalin and cortin, their results must be due to cortin rather than adrenalin effects. It has been shown, moreover, that the complete bilateral removal of the adrenal medulla or of the sympathetic nervous system does not reduce work capacity in the intact animal (13, 15, 29). In fact, Harris and Ingle (33) found a slight but significant increase in adreno-demedullated rats during moderate exercise. Conversely, work capacity is not increased in the intact animal by injection of adrenalin either before or during work (13). Actually, we gather from Campos *et al.*'s work curves (13, pp. 686, 688) that after adrenalin injection exhaustion set in earlier than normal; after the adrenal medulla was inactivated there is a decided *delay* in exhaustion and increase in the amount of work performed, corresponding to Harris and Ingle's results. It is possible that this increase is due to the antagonism between cortin and adrenalin action which has been suggested by Anderson *et al.* (2) on the basis of their experimental findings. As it has been found that cortin does increase work capacity in the intact organism (29) the elimination of its antagonist, adrenalin, would

explain the increase in muscular efficiency and endurance after removal of the adrenal medulla. At any rate we can take it as proved that adrenalin does not prevent fatigue in the intact organism, and the absence of adrenalin secretion or of sympathetic stimulation does not hasten it; rather, it may actually delay it.

In addition to these findings, there is a considerable body of experimental evidence showing that adrenalin actually diminishes muscular contractions in the intact organism. Thus Gellhorn, Darrow and Yesinick (27) found that adrenalin diminishes or prevents insulin and metrazol convulsions in rabbits, without return of the blood sugar to normal. Darrow and Gellhorn (20) report that it decreases the reflex excitability of the sympathetic nervous system and also reduces somatic hyperexcitability. Hall and Goldstone (31) found that shivering is suppressed by adrenalin even in very small concentrations. Floyd and Keele noted that it decreases the action potentials of the quadriceps of decerebrate animals, and Schweitzer and Wright that it diminishes the amplitude of the knee jerk (quoted by Hall, 31). Several authors have concluded that the most important component of adrenalin action is a central depressant effect (31). According to our own evidence (3), adrenalin injections prevented sound-induced seizures in susceptible rats, as well as in animals made susceptible by strychnine injections. It also reduced muscular tension and resulted in occasional prostration.

Thus we submit that there are grounds for maintaining that in the intact organism adrenalin secretion (in fear) or adrenalin injection diminishes muscular activity and reduces muscular contractions.

Against this evidence for the depressant effect of adrenalin we have to weigh Cannon's findings (confirmed by Bulbring and Burn, 12, and others) that adrenalin increases muscular contractions after fatigue in the nerve-muscle preparation, and Campos *et al.*'s report (13) that in the intact organism, after complete exhaustion, small doses of adrenalin (.02 mg./kg.) will restore the capacity to work and increase the amount of work performed up to 45 per cent. In both cases, these findings are of pharmacological rather than physiological interest. It is platitudinous to insist that the mechanism of action found in the nerve-muscle preparation is not necessarily employed by the intact organism; but even in the living organism, after exhaustion adrenalin may be supplied from the outside though *secretion* is impossible. For physiologically, it has been found that the adrenal medulla is depleted after severe exercise (17), and psychologically, the readiness to respond is diminished in exhaustion so that a formerly emotion-producing stimulus will no more be reacted to. This fact is illustrated by the feeble response of rats to teasing after a seizure, and also by Campos *et al.*'s failure to induce 'excitement' in their dog after exhaustion. The explanation of the energizing effect of adrenalin after exhaustion can be found, we believe, in Gellhorn's report (27) that the increase in blood pressure caused by adrenalin or sympathetic stimulation automatically produces parasympathetic excitation.[1] While normally this may be masked by the prolonged effects of previous sympathetic stimulation (*e.g.*, in fear), after exhaustion of the adrenal medulla an injection of a small dose of adrenalin would produce a profound parasympathetic effect. If, on the other hand, a large dose is given, there would be enough adrenalin present in the body to produce extensive sympathetic symptoms

[1] The significance of this will be dealt with later.

with a consequent masking of the parasympathetic reaction. Thus Campos *et al.* report that after exhaustion a large dose (.04 mg./kg.) 'excites' the dog, increases fatigue and prevents further work.

Vasoconstrictor effect of adrenalin.—And now we turn to the mechanisms by which adrenalin, according to Cannon, helps to put the body on an emergency footing.

In the first place, Cannon maintains that adrenalin causes vasoconstriction in the viscera (shown by a decrease of volume in various visceral organs) and in cutaneous vessels; but as limb volume was thought to be increased, he concluded that adrenalin (in emotion) "will drive the blood out of the vegetative organs of the interior, which serve the routine needs of the body, into the skeletal muscles which have to meet by extra action the urgent demands of struggle or escape" (14, p. 108). But it has been found since that "in stimulation of the splanchnic nerves the volume of the hind limb first increases passively with the rise of pressure and then diminishes to much below its previous volume. This diminution is due to the discharge of adrenalin into the blood stream and is absent if the suprarenals have been previously destroyed" (23, p. 690). Moreover, as it is now possible to measure the flow of blood directly, by means of the stromuhr, instead of inferring it from the change in limb volume, we possess now direct evidence on this question, which was not available to Cannon at the time. Thus Mertens *et al.* who measured the blood flow in resting skeletal muscles by the stromuhr method, report that the injection of adrenalin produced a pronounced vasoconstriction, and *reduced* the blood flow. However, "it is found that adrenalin which readily decreases the blood flow in the resting muscle, loses this action when the muscle is in a state of activity, or shows an increased blood flow due to the previous injection of acetylocholine" (Mertens *et al.*, quoted by Gellhorn, 27, p. 236). Thus the adrenalin effect has to be overcome by muscular activity before an increased blood flow can meet the increased oxygen need of skeletal muscle.

Glycogenolytic action of adrenalin.—Cannon rests his claim of a dynamogenic action of adrenalin also on the fact that adrenalin liberates glycogen from the liver and thus increases blood sugar. But adrenalin also decreases the glycogen store in muscle and increases lactic acid formation; after injection of adrenalin in large as well as in small (physiological) doses definite reduction of the glycogen store in muscles was observed (45). According to Cameron (17) and Verzar (55), this action of adrenalin is a purely lytic one. Glycogenesis in the liver apparently requires the presence of cortin (55, 11) while the formation of glycogen in muscle is increased by insulin (11). There seems to be no evidence that adrenalin is involved in the formation of either muscle or liver glycogen.

Though the glycogen withdrawn from the muscles in the form of lactic acid can be converted again into glycogen by increased oxygen consumption, in strenuous exercise as well as after adrenalin injection the oxygen requirements will finally exceed the oxygen intake with the result that muscle glycogen is found to have decreased while lactic acid has accumulated. If these changes are in the same direction of increased energy expenditure, both during strenuous exercise and after adrenalin injection, so that either of them alone will lead to prostration, how can fear or adrenalin provide *additional* energy? Rather, the energy expenditure produced by adrenalin secretion or injection could be expected to interfere with or detract from the energy expenditure during strenuous muscular effort.

Effect of adrenalin on the heart rate.—Cannon also maintains that the increase in heart rate produced by adrenalin materially aids in preparing the organism for emergency action. He shows, for instance (15, p. 152), that in a cat the rate of the denervated heart was accelerated from 120 to 160 per minute after excitement as long as the adrenals were active, while it only increased from 118 to 120 after they had been inactivated. On inspection of his graphs we find that the force of contraction of the heart, *i.e.*, the stroke volume, is almost twice as high after the adrenals were inactivated as it was with adrenals active. Thus the net effect would be an increase in cardiac output with inactive adrenals in spite of the decrease in rate. Characteristically, also, the increase in work capacity reported by Campos and Cannon (13) when adrenalin was injected after exhaustion, was not accompanied by an increase in heart rate, and in some cases with a decrease.

Borgard (10) has shown that in trained athletes strenuous exercise results in a simultaneous increase of heart rate, stroke volume and peripheral utilization of oxygen. Both at rest and at work, the trained athlete has a considerably *lower* heart rate than his untrained fellow, who increases his heart rate conspicuously during exercise without corresponding increase of the other two factors. Thus unbridled adrenalin action which increases the heart rate without increasing stroke volume and oxygen utilization, would actually be a hindrance rather than a help to forceful action.

Adrenalin and polycythemia.—Cannon finally claims that as a further emergency measure the spleen contracts during sympathetic stimulation, discharging its content of red blood corpuscles into the blood stream. Barcroft's findings (5) that a stimulation of the splanchnic nerves produced contraction of the spleen, and Izquierdo and Cannon's findings (40) that polycythemia was observed during rage, form the basis for this conclusion. But it is more than likely that in stimulating the splanchnic nerve the parasympathetic fibres to the spleen were stimulated at the same time (*e.g.*, Kure's spinal parasympathetic fibres, 42). As acetylcholine, the parasympathetic mediator, has been found to produce contraction of the spleen (9) and such contraction also appears after vagus stimulation (Roy, quoted by Barcroft, 5) it seems possible that it represents a cholinergic response. This would further be substantiated by the fact that Dale (quoted by Cannon and Rosenblueth, 16) found considerable acetylcholine present in the horse spleen. Contraction of the spleen is also produced by emotional stimuli, especially by startle (32) and rage (4, 32), but none of the reactions produced could be called fear reactions. As startle has been shown to be accompanied by cholinergic excitation (7, 27), our suggestion seems to gain weight.

The adrenalin effect on the spleen could then be explained as a secondary cholinergic reaction (*cf.*, p. 41). Actually, Izquierdo and Cannon (40) report that adrenalin injection produces in some cases a slow and soon subsiding contraction of the denervated spleen, different from the instantaneous reaction after excitement, while in other cases there is no constriction at all. The removal of the adrenal medulla does not prevent constriction in animals with innervated spleen during rage. That the contraction of the spleen after adrenalin is a cholinergic phenomenon is also confirmed by Hoet's observation (quoted by Cannon and Rosenblueth, 16, p. 162) that it is abolished by atropine, the parasympathetic antagonist.

As Gellhorn and Feldman (27) have shown that during exposure to low tem-

peratures sympathetic stimulation predominates while warm temperatures act predominantly on the parasympathetic system, the increase in red blood corpuscles which has been observed among people living in warm climates, usually ascribed to splenic contraction, and their decrease upon return to colder regions (9) can be taken as further evidence supporting our suggestion.

Heat regulation.—The question of heat regulation is interesting in itself in view of Cannon's theory of homeostasis. He claims that the sympathetico-adrenal mechanism protects the organism from the effect of cold by vasoconstriction, thus lessening the exposure of blood to the surface. But surely we are not protected against cold by a prolongation of the initial vasoconstriction but by the organism's reaction to it. Thus cutaneous vessels dilate after an initial constriction when the hand is put into cold water; and the discomfort of exposure to cold lessens as soon as exercise increases blood flow (*cf.*, p. 37) and brings about vasodilatation. An obvious example of such a reaction is the ruddy complexion of people in cool climates as contrasted with tropical pallor. Thus we are protected against cold not by the sympathetic vasoconstrictor action but by vasodilator reaction. Whenever this reaction fails or is insufficient, cold becomes harmful (*e.g.*, in Raynaud's disease).

Significantly enough, Cannon's sympathectomized monkey (15) died of heat prostration, for the sympathetic reaction was not available, while none of his sympathectomized cats was any the worse for its exposure to cold, for increased shivering, he reports, enabled them to maintain constant though fairly low temperatures.

Our hypothesis would also explain Maier and Glaser's findings (44) that sound-induced seizures tend to be inhibited if the animals have been exposed to swimming in cold water until there is a definite fall in body temperature. As during exposure to cold the sympathetic effect predominates and in this case was not sufficiently compensated for by exercise (as indicated by the fall in body temperature), seizures would be prevented by the effects of sympathetico-adrenal stimulation.

Looking back over the physiological evidence presented, it would seem to us that there is little to support a 'dynamogenic' action of adrenalin, whether secreted or injected, and much to explain its depressant effects, as noted by us and other investigators (*cf.*, p. 36).

Psychological effects of fear.—If we now turn to the psychological evidence for the effect of fear on the organism, we notice that in every instance where fear symptoms are described, the level of somatic activity is reduced (30, 53) in sharp contrast to anger, where it is raised (53). Analogously, it was found that adrenalin injection decreased the activity of normal rats (47). We find further that the rats of Hall's emotional strain which show the symptoms of fear in abundance are more timid and less aggressive (30), which does not support Cannon's contention that adrenalin secretion in emotion prepares for *fight* or flight. It does not solve our problem to say that fear may be paralyzing if attack or flight is impossible (14): for introspectively, extreme fear is paralyzing before the possibility of flight can be explored, and experimentally, the level of activity is reduced in a fear situation (*e.g.*, with rats in the open field situation or a food incentive maze) even though either flight to the living cage or 'attack' of the situation (exploration of maze or open field) is equally possible. Rather, the fact seems to be that in mild fear muscular activity is merely reduced while in extreme fear it is prevented altogether. This seems to be supported by our findings (3) of a reduction of

activity in two-minute sound stimulations, and complete prostration at the end of a prolonged stimulation period.

In view of the evidence presented we conclude, therefore, that fear must be enervating rather than energizing. Fear may be useful to the organism, indeed, not because it prepares us for action but because it forces us to caution. Completely fearless, the human race as well as its animal progenitors would have destroyed themselves long ago. With fear came the realization of future danger, and, perhaps, the dawn of consciousness. The penalty we pay for an increased awareness is a retardation of action. Though we succeed in acting in spite of the fear which threatens to paralyze us, we move with effort and miss the accustomed ease of action. Only to the extent to which we overcome fear, forget ourselves, do we regain our sureness of action, our efficiency. From mental blocking and stage fright to chronic anxiety reactions we can trace the connection between fear and consciousness: as soon as we forego the consciousness of our action, forget the dangers which threaten us or our self-esteem, as soon as we can lose ourselves in action, we have conquered fear. But if, instead of acting, we succumb to fear, action will soon become impossible.

The feats of endurance Cannon describes as a result of fright may perhaps be compared to a startle response (*cf.*, p. 46), rather than to a conscious experience of fear. William James realized something like that when he maintained that we run and *therefore* are afraid. We would paraphrase this by saying that we run *before* we are really afraid: but as we run we begin to realize our danger and become more and more afraid, until we collapse with shaking knees and pounding heart, far more exhausted than we should be from the muscular effort alone.

Summing up we would suggest that the emergency theory of emotion does not seem to hold good for the emotion of fear. Neither can the effects of fear be explained as an adaptive reaction by which the organism restores its internal equilibrium (homeostasis).

Cannon's theory of homeostasis.—Cannon's theory of homeostasis (15) was the logical extension of his emergency theory of emotion. Both interpretations are based on the assumption that every phenomenon observable in the organism after environmental interference is an 'adaptive reaction' and therefore a survival reaction. But if the organism always automatically selects the best possible reaction for survival, how can we explain the host of obviously nonsurvival reactions which are just as well known in physiology as in psychology? The shock reaction, or the proliferation of cancer cells, or a thrombosis, obviously do not make for survival —as little as an anxiety neurosis or a psychosis, yet they are 'adaptive reactions' in Cannon's sense. Our attempts at therapy are based on the fact that only an interference with the processes going on in the organism during disease will prevent reversible changes from becoming irreversible. Thus we have to accept the fact that some processes, though 'adaptive' in Cannon's sense because they are the result of a disturbance of the organism's equilibrium, do not contribute to the well-being of the organism as a whole.

We venture to suggest that the confusion between 'adaptive reactions' and 'survival reactions' could be avoided if we distinguished between the stimulus *effect* on the organism and the organism's *reaction* to it. Thus the formation of a blood clot in a blood vessel would be the effect of some disturbance, while the subsequent absorption would represent the organism's reaction. Some types of environmental interference seem to result in a vicious circle effect against

which the organism cannot set up an efficient reaction. Thus hemorrhage leads to lowered blood pressure producing sympathetic excitation (probably via the carotid sinus reflex), which results in a decrease in blood volume because of greater permeability of capillaries. This in turn reduces the circulating blood volume, which tends to aggravate the fall in blood pressure still further until the animal dies in shock. When this vicious circle reaction is prevented by sympathectomy or ergotoxin (which blocks sympathetic excitation) shock does not develop even though the blood pressure is lower than in normal animals (27, p. 263).

Our hypothesis, applied to the autonomic nervous system, would result in the assumption of alternating action and reaction between sympathetic and parasympathetic systems, instead of the orthodox assumption of reciprocal innervation. Gellhorn's investigations have shown that the assumption of strict reciprocal innervation is untenable because during sympathetic excitation parasympathetic symptoms appear whenever sympathetico-adrenal effects are blocked; he therefore assumes simultaneous excitation of sympathetic and parasympathetic systems. Actually, our assumption would fit Gellhorn's facts and would in addition explain most of the contradictory evidence in the field. We should have to assume, *e.g.*, that vasoconstriction and rise in blood pressure is the sympathetic *effect* of cold or adrenalin, which will immediately result in a parasympathetic *reaction*. Furthermore, it would be reasonable to suppose that the larger the dose of adrenalin, or the more extreme the cold stimulation, the more will sympathetic effects predominate; the smaller the dose, the less will be the sympathetic *effect* and the more noticeable will be the parasympathetic *reaction*. This would explain, *e.g.*, why with small doses of adrenalin (up to 10 gamma/kg.) cardiac responses to vagal stimulation are increased, *i.e.*, the heart rate decreases; and if more than 20 gamma/kg. adrenalin is injected, the influence of the vagi is reduced or even abolished (16). We could further assume that as soon as the sympathetic *effects* are excessive, exhaustion of the humoral mechanism will supervene and the parasympathetic *reaction* only will be noted. Thus Feldberg (24) has shown that in the eserinized dog with an excess of adrenalin in the circulation (previous injection of 2 to 3 mg.) stimulation of the splanchnics will *only* cause a fall in blood pressure.

Similarly, a stimulation of the parasympathetic system, *e.g.*, during heat, will represent the *effect* of a disturbance of the organism's equilibrium, which is *reacted to* by the sympathetic system, and this reaction (not the primary effect) will restore the organismic balance. This hypothesis would explain Gellhorn's findings (27) that on exposure to heat parasympathetic activity predominates though sympathetic activity becomes noticeable on section of the vagi; while on exposure to cold sympathetic activity predominates though vago-insulin activity becomes apparent as soon as the sympathetico-adrenal system is blocked. Gellhorn's assumption of simultaneous excitation of both branches of the autonomic nervous system demands an explanation of the dominance of the sympathetic system during cold stimulation, and the dominance of the parasympathetic system during heat stimulation. Our hypothesis which differentiates the *effect* of stimulation from the organismic *reaction to* it, distinguishes at the outset between primary and secondary excitation. On exposure to heat, the *parasympathetic effect* is more pronounced than the *sympathetic reaction*. In other words, in this case parasympathetic excitation will be primary, sympathetic excitation secondary.

Exposure to cold, on the other hand, produces a pronounced *sympathetic effect* (blanching of the skin, contraction of bloodvessels, etc.) which will mask the simultaneous *parasympathetic reaction*. In this case, then, sympathetic excitation will be primary, parasympathetic activity secondary. We should expect that immediately *after* exposure to heat the sympathetic reaction should become noticeable, just as the parasympathetic reaction would become apparent *after* exposure to cold.

There is considerable evidence for such a mechanism of alternative activation, *e.g.*, the carotid sinus reflex by which an increase in blood pressure in the carotid sinus area and the aortic arch (brought about by adrenalin, sympathetic stimulation or mechanical means) will produce parasympathetic excitation, while a decrease in blood pressure produces sympathetic excitation (27, p. 219). There seems to be a possibility that this alternating action and reaction is not only achieved by a nervous but also by a humoral mechanism. Thus intravenous injection of acetylcholine produces reflex liberation of adrenalin and cardiac acceleration (56). Giordano and Zeglio (28) report that injection of insulin results in increase in blood adrenalin—and the secretion of insulin is increased by parasympathetic stimulation.

Our hypothesis can also explain diphasic or paradoxical reactions after administration of adrenalin. Thus Templeton and Lawson (52) report that adrenalin has a diphasic action on the large intestine, first depression, later augmentation of peristalsis; the second phase (augmentation) is increased by pilocarpine, abolished by atropine. This would mean that the adrenalin effect set off a cholinergic reaction—which is confirmed by the fact that pilocarpine augmented and atropine abolished it. According to Bender (7), intravenous injection of acetylcholine produces an inconstant constriction and a more frequent secondary dilatation in the denervated iris of the cat, while in the monkey there is only constriction. Intravenous injections of adrenalin produce a dilatation in the cat, but a delayed constriction in the monkey. These species differences can be explained by the fact (7) that in the cat the sympathetico-adrenal system predominates, while the parasympathetic system is dominant in the monkey. Bender reports that pupillary constriction after adrenalin in cat or monkey is increased by eserine. As eserine prevents the esterase from destroying the parasympathetic mediator acetylcholine, and thus enhances parasympathetic excitation, it would seem that such pupillary constriction after adrenalin represents a secondary parasympathetic reaction to the primary sympathetic excitation. Bender also finds that the pupillary dilatation after acetylcholine is usually abolished by bilateral adrenalectomy. This again would show that the acetylcholine effect (consisting in pupillary constriction) is reacted to by the sympathetico-adrenal system producing secondary dilatation. This dilatation may occur in many cases in spite of adrenalectomy because the sympathetic reaction will not only result in secretion of adrenalin but also in the production of sympathin at the myoneural junction.

If sympathetic excitation is blocked by ergotoxin, adrenalin produces a so-called reversal effect: instead of a rise in blood pressure, a fall is the result (1, 48, and others). However, Bacq (4) has shown that ergotoxin itself produces pronounced sympathetic effects, although it blocks further stimulation. Thus ergotoxin produces a rise in blood pressure although now there will be no further rise due to adrenalin injection or sympathetic stimulation. According to our hypothesis, a subsequent injection of adrenalin (or stimulation of sympa-

thetic nerves) would represent excess stimulation (*cf.*, p. 41) and thus result in exhaustion of the sympathetic mechanism, so that instead of a further sympathetic effect (further rise of blood pressure), only the parasympathetic reaction (fall in blood pressure) would be noticed.

Energizing emotions. Now that we have made this digression, we shall investigate whether there are emotions which are not as exclusively identified with sympathetic effects as fear seems to be—in other words, whether there are energizing as well as enervating emotions.

Stratton (51) has described one emotion which could lay claim to this distinction. Excitement (or elation), as he describes it, is an emotional state (neither anger nor fear), easily recognized in human experience, which sharpens our perceptions and heightens our endurance. Unfortunately, there is so far little direct experimental evidence which would allow us to suggest a physiological correlate.

Secondly, we have to consider anger, which has been shown to raise the level of general activity (53), and is usually accompanied by increased muscular tension. It may well be that the felt difference between acute anger and acute fear (flushing instead of pallor, violent aggressive movements instead of tremor and cowering, a feeling of tension and 'fullness' instead of a feeling of weakness), is not as superficial as Cannon considers it to be. There is a possibility that anger may be accompanied by parasympathetic excitation just as fear is accompanied by sympathetic stimulation. Not only are most of the symptoms mentioned above of parasympathetic origin but there is also some physiological evidence which would lend weight to such a suggestion. Gellhorn, Cortell and Feldman (27, p. 198) produced rage in cordotomized cats by confronting them with a barking dog, and sham rage in adrenalectomized cats and rats by stimulation of the hypothalamus. The result was a parasympathetically induced hypoglycemic effect which could be abolished by section of the vagi. Gellhorn (27, p. 301) also reports that in psychotic patients during strong excitement there is an increased insulin concentration in the blood, produced by parasympathetic excitation. Gellhorn concludes from these observations that excitement results in simultaneous sympathetic and parasympathetic excitation whether in normal or psychotic subjects. As, however, sympathetic reactivity has been found to be reduced in psychotics, Gellhorn assumes that in such patients parasympathetic activity will predominate over sympathetic activity, resulting in increased insulin secretion (27, p. 300). But it is at least curious that the symptoms of 'excitement' in psychotics as far as their motor manifestations go, are indistinguishable from violent rage responses of normal people; if sympathetic activity were the most important component, we should expect a reduction in the emotion as soon as sympathetic activity becomes sluggish. Surely it is just as likely that in psychotic patients the sympathetic *reaction* to parasympathetic effects of excitement (or anger) is depressed and therefore the parasympathetic effect predominates while in normals the sympathetic reaction via the carotid sinus reflex eventually would produce a predominantly sympathetic blood picture. The above conjecture would be confirmed by Pfister's finding (27, p. 300) that the carotid sinus reflexes are weakened in schizophrenic patients. It is also significant that in schizophrenic as well as in manic patients (both types show parasympathetic effects during excitement) one hardly, if ever, sees any symptoms of fear while anger and excitement are considerably stronger than in normals. If our inter-

pretation is correct, we should expect that removal of the buffer nerves, by blocking the carotid sinus reflex, would show increased parasympathetic effects in, *e.g.*, an enraged dog (the comparatively slight motor response of an enraged cat may quite possibly be due to the fact that in this species the adrenalin reaction predominates [*cf.* 7]). To our knowledge such an experiment has so far not been performed.

That anger is accompanied by parasympathetic excitation is further proved by the fact that anger has been reported to have caused biliary colic (8) and the cessation of bile flow (22) owing to spasm of the biliary duct—both parasympathetic phenomena. Tears, which flow abundantly during temper tantrums, are inhibited in fear—and the secretion of tears is a result of parasympathetic stimulation (43). Emotional defecation which is abundant during fear (30, 53) and ceases during anger (53) may be due to a relaxation of the sphincter ani as a result of sympathetic stimulation, for Delmas and Laux (21) have reported that blocking of the sacral (parasympathetic) nerves resulted in relaxation of pelvic sphincters. Urination, which occurs both in fear and anger (53), may be caused by the same mechanism of sympathetic relaxation of the sphincter in fear, but as it also occurs reflexly after contraction of the bladder (26) owing to parasympathetic stimulation, its occurrence in anger may be caused by the latter mechanism.

Finally, the pilomotor reaction, which is very pronounced in rage, has been shown by Rothman and Coon (50) to occur in response to acetylcholine as well as to adrenalin. In fact, the acetylcholine response seems to be much quicker and more pronounced, while the response to adrenalin is slow in onset and more moderate. Is it unreasonable to suggest that the so-called 'adrenalin response' may be the secondary parasympathetic reaction to the adrenalin effect on the organism? Thus the almost instantaneous pilomotor response in rage would seem to point to parasympathetic rather than to sympathetic stimulation. That adrenalin, at any rate, is not necessary for a vigorous pilomotor response in rage has been shown by Gellhorn, Cortell and Feldman (27) who found such a response in animals after removal of the adrenals.

According to our hypothesis, every parasympathetic excitation (*e.g.*, in anger) will produce a sympathetic reaction and with it a discharge of adrenalin, though not necessarily a fear reaction. This may explain not only that previous investigators invariably found an adrenalin response in rage but also that extreme rage becomes paralyzing—the more so the longer it lasts. "Trembling with rage" and being "white with anger" would, on this interpretation, represent secondary sympathetic reactions with attendant adrenalin secretion, without being fear experiences. That such a dissociation of adrenalin effect and fear is possible has been shown repeatedly (36, 46).

Such secondary reaction of the sympathetico-adrenal system would also seem to be the explanation of the fact that evercise (which is accompanied by increased acetylcholine concentration) results in increased adrenalin secretion. It may be that the adrenalin then contributes to fatigue by its depressant effects, especially as it has been found (49) that the adreno-cortical sterols, which are instrumental in maintaining muscular efficiency, are reduced in muscles after exhausting exercise, with a simultaneous increase in adrenalin concentration. Training would then postpone fatigue by eliminating undue sympathetic reaction.

Vice versa, the sympathetic effects of fear would result in a parasympathetic reaction. With a release from fear af-

ter prolonged stimulation, the parasympathetic reaction will become apparent in the form of so-called 'release-phenomena': crying, laughter, etc.

And now perhaps we have a clue to Stratton's emotion of 'excitement' or elation. May it not be the result of moderate parasympathetic excitation which is not strong enough to stimulate vigorous sympathetic reaction with its adrenalin secretion, but will increase gastric secretion and peristalsis, and will permit smooth transmission of impulses in the central nervous system. In support of this suggestion we might mention the results of several investigators (quoted by Gellhorn, 27, p. 194), who found that weak hypothalamic stimulation increased peristalsis and produced a slight fall in blood pressure (*i.e.*, produced parasympathetic symptoms) while a strong stimulus inhibited peristalsis, raised blood pressure and produced other sympathetic symptoms.

Such moderate parasympathetic stimulation with its influence on insulin secretion seems to be responsible for the dynamogenic action of the autonomic nervous system, while adrenalin reaction is only required if the blood sugar is lowered by excessive vago-insulin stimulation (*e.g.*, in anger). Actually, it has been shown (27, p. 270) that insulin is necessary for maintenance of the normal blood sugar curve, while adrenalin is not.

As for corresponding psychological evidence, we should like to mention the report of a careful investigation by Darling (18) and Darling and Darrow (19) who showed that individuals whose reactions were preponderantly parasympathetic, were 'more alert, more excitable or non-inhibited and more active physically,' while preponderantly adrenergic individuals 'tend to be rated distractible, uncoöperative, somnolent, timid, inhibited and sluggish.' Factor analysis of the data was in agreement with the above conclusions.

Relation of anger to explosive reactions.—And now we can investigate the possibility that there is a more than casual connection between anger and sound-produced seizures, both of which are distinguished by a violent motor pattern.

Tseng (53) for instance has found that in a group of 19 rats previously not susceptible to seizure ten proved to be susceptible after a two-weeks period of anger stimulation. Of a comparable group of 16 rats, left in their living cages, only one had attacks at a later retest.

That this finding has a parallel in clinical experience is shown by the well-known fact that human epileptics are inclined to irritability and temper tantrums, and that anger or excitement are frequent precursors of attacks. Timidity and fearfulness, however, are hardly, if ever, mentioned as components of the 'epileptic temperament.' There is a similar connection pointed out by Kardiner between epileptoid reactions in traumatic war neuroses (panic states) and anger:

> The aggression may show itself in the tendency to tempers. Easily aroused to anger, these patients are very prone to motor expression. They either break or tear objects in these fits of temper or strike the people who happen to be around them. . . . If the outburst is accompanied by loss of consciousness, the patient is usually dangerous (41, p. 95).

The same patients exhibit explosive reactions to noise, but no fear, as shown in another quotation:

> These patients, in response to sudden auditory stimuli, do not go into anxious states, but into fright reactions. The responses are chaotic, at times paroxysmal, and lack organization; after the reaction is over, there remains no residue of anxiety but only a heightened sensitivity to the

stimulus. . . . As a rule, no anticipatory fear . . . exists in these fright reactions to noise (41, p. 95).

If we now admit the possibility that anger may be accompanied by parasympathetic discharge and may be related to explosive reactions, Humphrey's (34, 35) findings become significant. If, as he finds, seizures can be produced by eserine (which preserves the parasympathetic mediator acetylcholine), though apparently not by its action on parasympathetic effectors, we must assume that these seizures are produced by its facilitating effect on the central nervous system. It has been found, indeed, that acetylcholine is instrumental in the transmission of nerve impulses not only in the parasympathetic system but also in the central nervous system (23, 27). Thus we could assume that temper tantrums for instance would represent a cholinergic discharge of nervous excitation primarily within the parasympathetic system, while the explosive reaction of the sound-induced seizure would represent a cholinergic discharge primarily via the motor system.

An even closer relationship seems to exist between the explosive reaction and startle. Startle is known to be a parasympathetic phenomenon (7, 27) and has actually been found to be increased considerably in susceptible rats (54). The connection of anger, and fright reactions to noise, has also been pointed out by Kardiner, quoted above (p. 45).

We would suggest that the startle reaction to the bell may either be intense, indicating a strong and continued cholinergic excitation which reaches its climax in the sound-produced seizure; or the startle response may be slighter, indicating comparatively moderate cholinergic excitation which can be reacted to by the sympathetico-adrenal system sufficiently strongly to depress motor excitation and increase 'involuntary' activity (tremor, twitching, lip-wetting). The degree of cholinergic excitation which can be reacted to effectively will depend on the degree of irritability of the sympathetic nervous system in the individual organism at the time of sound stimulation. We should have to assume that normal animals (*i.e.*, rats not subject to sound-produced seizures) possess a moderate degree of parasympathetic excitability as compared with susceptible animals.

SUMMARY

We believe that there are at least three different physiological states corresponding to three different emotions: *fear*, with predominantly sympathetic excitation; *anger*, with strong parasympathetic excitation; and *excitement* or elation, with moderate parasympathetic activity. It would depend on the autonomic reactivity of the individual and the intensity of his emotional experience whether the reaction to the emotional effects will be noticeable during emotional stimulation or after it.

We also distinguish two excitatory states which we hesitate to call emotions (startle and explosive or epileptoid reactions), for both seem to represent a short circuit—what has been called a 'functional decortication' (6)—via the cholinergic mechanism of the parasympathetic and central nervous systems. In both cases, a 'conscious' evaluation of the stimulus situation seems to be missing.

We suggest further that the most favorable conditions for activity are found in a moderate stimulation of the cholinergic mechanism, resulting in facilitation of action by 'excitement' without untoward secondary adrenalin effects (at least for the duration of the activity [2]).

[2] The well-known 'let-down' after excitement could be considered as secondary sympathetic reaction which becomes apparent as the parasympathetic stimulation subsides.

Neither anger nor fear can be shown to have an emergency function. Rather they represent obstacles to efficient action, the former by short-circuiting too large an amount of excitation into the parasympathetic system, the latter by inundating the sympathetic system.

Amending Cannon's theory of homeostasis we would suggest, therefore, that during overactivity of the sympathetic division of the autonomic nervous system (in fear or cold) the balance of the organism is restored by a cholinergic *reaction* in the form of activity; and during parasympathetic overactivity (in anger or during heat) by a secondary sympathetic *reaction* with its adrenergic effects. If such a reaction is delayed or insufficient, or if the original stimulus is protracted too long, the primary effects of overstimulation may become irreversible.

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WHAT FIXES THE CORRECT RESPONSE?

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In a recent article (3), 'The effect of delayed reinforcement upon the differentiation of bar responses in white rats,' C. T. Perin included a highly significant footnote which might tend to be overlooked. This footnote opens the possibility of an interpretation of the experimental results very different from that of Mr. Perin.

Perin trained 150 albino rats to push a bar, food being presented immediately subsequent to their pressing of the bar in either direction. After this preliminary training, the rats were divided into six groups and then received the food only after pressing the bar in the 'correct' direction: that is to say, in the direction least used by the rat during the preliminary training. Also, when the bar was pressed in the correct direction, it now was withdrawn immediately. The six groups of rats differed only in the length of delay interval between the correct bar response and the food presentation, these intervals ranging from 0 seconds, through 2, 5, and 10 seconds, to 20 and 30 seconds.

Under these conditions, rats with a 0-second delay learn very well, rats with a 2- or 5-second delay learn only slightly less well, rats with a 10- or 20-second delay learn relatively poorly, and those with a 30-second delay show very little learning, extinguishing readily. Perin remarks that "a marked decrease in the ultimate strength of the habit is not noticeable until the delay period is as long as 10 or 20 seconds" (3, p. 105), with the criterion of habit strength consisting of the percentage of trials in which the first response made was the correct response. This lack of any marked differences among the 0-second, 2-second, and 5-second delay groups is accounted for by Perin on the basis of these factors: (1) The simplicity of the task was so great that a level of 100 per cent perfect trials was reached by all three groups; and (2) the preliminary training, in which both the correct and incorrect responses were reinforced, gave the required correct response "sufficient habit strength both through direct reinforcement and through generalization from the previously reinforced incorrect response for it to be learned to an almost perfect degree even when the reinforcement for that response was delayed in the later differentiation training" (3, p. 106). Perin concludes that "the rate of habit acquisition is an inverse function of the delay-of-reinforcement interval" (3, p. 109).

Perin's conscientious reporting of the experiment leads him to include in a footnote certain facts of the situation which to him were only an annoyance, but to the writer suggest a re-interpretation of the whole experiment. The footnote follows (3, p. 99):

> In the original plan of the experiment, provision was made for the bar to withdraw following both the correct and incorrect responses. With this arrangement the only difference resulting between responses was the delivery of food following the correct response. However, preliminary experimentation indicated that a differentiation of response under these conditions is very difficult, several animals having a food delay of 2 to 5 seconds showing no signs of learning the correct response. With some animals, several hundred incorrect responses were made in succession without the giving of any food reward. . . .

Guthrie (2, p. 111; 1, p. 157) has suggested that the preservation of a re-

sponse depends upon removal of the animal from the situation following that response. The rationale of this lies in the fact that such removal prevents new associations with the situation because the situation is then absent. The response last made in the situation is fixed, for no unlearning is possible after the animal is removed. Therefore, upon being placed again in the situation, the animal is expected to do what it last did in that situation, whether that was 'correct' or 'incorrect.'

On this hypothesis, food presentation constitutes partial removal of the animal from the situation, and upon this change in situation rests its efficacy. Bar-withdrawal also constitutes partial removal of the animal from the situation, and thus preserves whatever response preceded the withdrawal. Therefore, it would be predicted that if the bar were withdrawn following the incorrect response, that response would persist. If, however, it were withdrawn following the correct response, that response would persist.

Perin's results strongly suggest complete confirmation of this prediction. He reports that when the bar was withdrawn following all responses, some rats obstinately made the 'incorrect' response several hundred times, while other rats performed 'correctly.' Perin was unable to account adequately for this repetition of the incorrect response when no food was given for it. However, such behavior is exactly what would be expected on the basis of our theory of learning. If the rat when first placed in the situation made an incorrect response and the bar was then removed, the rat would make that same incorrect response when the bar was next presented. And he would continue to do so as long as there was no opportunity for unlearning that response and attaching a new response to the bar situation. If, on the other hand, the rat happened to make the correct response when first placed in the situation and the bar was then removed, this response would persist, and persist for the same reason as would the incorrect response in the other rats. Unfortunately Perin failed to note whether those rats which performed correctly were those happening to start with the correct response. He mentions only that learning of the correct response was very difficult, some rats responding incorrectly hundreds of times in succession. So far as Perin's report goes, it is quite possible *all* rats persisted in the response that preceded bar-withdrawal. And this is what would be predicted if situation removal does preserve a response through prevention of unlearning.

From the hypothesis stressing situation removal, it likewise follows that if the bar were withdrawn only after a correct response, that response would come to be the only response made to the bar. 'Learning' would occur. This prediction too is confirmed by Perin's experimental evidence. The rats in the 0-, 2- and 5-second delay groups reached a nearly perfect level of performance. Although Perin calls attention to the fact (3, p. 99) that bar-removal after the correct response excluded "the possibility of the occurrence of additional responses following the one correct response on a given trial," and mentions (3, p. 99) that "the bar remained in place during the occurrence of incorrect responses preceding the correct one," he apparently fails to note the significance of this. It is these added responses following the incorrect responses which make possible new associations with the situation. The incorrect responses can be eliminated *only* if the animal is left in the situation with the bar while he makes some other response to the bar. It is then the incorrect responses are unlearned, and the new response associated with the bar. In this way the eventual substitution of

the 'correct' for 'incorrect' behavior is made possible.

There is one further prediction. As mentioned previously, both bar-withdrawal and food presentation are means of partially removing the animal from the situation. Both make sudden, radical changes in the situation. In the case of the food, the animal begins to eat, which behavior changes his whole pattern of proprioception. It is for this reason that food presentation also constitutes partial removal of the animal from the situation. And it would be predicted that giving food following a response would tend to preserve that response. However, if withdrawing the bar effectively prevents unlearning a response to the bar, it would be expected that this would mask the effect of other slight changes in removing the animal from the situation. That is precisely what Perin found. Limits of habit perfection did not discriminate between the 0-, 2- and 5-second delay groups. But the longer delay groups were markedly inferior.

What is the result of increasing the interval between a correct response and its food reward? It is simply this: There is an increased opportunity for association of new behavior with the situation. The new behavior, in this experiment, will involve no bar responses. The bar is gone. Therefore, if the animal is left in the box after the bar is withdrawn, he will make new non-push-the-bar responses. Eventually one of these non-push-the-bar responses is followed by food presentation. This preserves the final non-push-the-bar response, and 'extinguishes' the bar response. With increased food delay, there would be an increased tendency for the animals not to learn the correct bar response. But neither would they learn the incorrect bar response. By hypothesis, they would simply cease making bar responses at all. This again is precisely what Perin found. In the 30-second delay group, 9 of 25 animals were extinguished, 'ceased to make bar responses' (3, p. 104). Three of the animals in the 20-second delay group had this fate, and none with shorter delays.

On the basis of the evidence from Mr. Perin's experiment and its footnote, this new interpretation and conclusion seem justified: Learning is not an inverse function of the length of the delayed food interval as such. Learning is, rather, a function of the removal of the animal from the situation following some response. That response consequently is preserved and repeated through prevention of its being unlearned.

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A REPLY TO WYATT AND TEUBER [1]

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In their interesting study, 'German psychology under the Nazi system: 1933–1940' (2), Wyatt and Teuber refer to my article on 'Pathological Nazi stereotypes found in recent German technical journals' (1). Specifically, they remark:

> He observes a sharp deterioration in the quality of German psychological writing as the expression of mental confusion and severe disturbances of mental health. Without doubting the latter, it must be said that *the author's selection tends to be misleading* (italics mine). Of 35 papers mentioned, only four were published in the *Zeitschrift für Psychologie*, the best reputed journal in the field, whereas 16 of the papers mentioned originated from the *Zeitschrift für pädagogische Psychologie* which, from the point of view of scientific psychology, was always considered to be somewhat 'marginal.' The tradition of this periodical, its readers and its editors account for the docility toward the NS [national-socialistic] doctrine and the new style in science (2, p. 239, footnote 11).

Explicitly, the authors are in error in making this statement. Implicitly, the nature of this error tends to weaken certain important inferences about problems of German re-education. The reader is asked to consider the following facts in this connection.

1. As I clearly stated in my introductory paragraphs, "the excerpts were all culled from the *Psychological Abstracts* published by the American Psychological Association" (1, p. 179). I added, in the same place, that "only those articles were utilized which contained explicit and unequivocal forms of Nazi stereotypes." On this basis, "only articles abstracted from *professional* (italics mine) journals were included beginning with January, 1939, through and including November, 1941" (1, p. 179). Note that the basic selection is not mine, but that of the *Psychological Abstracts*, official abstracting organ of the American Psychological Association. I could, or at least did, only follow the selection made by the abstracting staff of the *Abstracts*. Note further, in connection with Wyatt and Teuber's emphasis on "scientific psychology," that I limited my secondary selection (following the primary selection of the *Abstracts*) to *professional* or *technical* journals—"supposedly dealing with problems and techniques of scientific psychology, psychotherapy, pedagogy, neurology, and psychiatry" (1, pp. 181–182). Note that in enumerating the professional journals involved, I clearly distinguish between those devoted to pure 'scientific psychology' and those dealing with the applied psychological and psycho-technical arts. Nowhere is the claim made that the citations are only from journals in the field of pure or scientific psychology.

2. In my article I repeatedly and explicitly called the readers' attention to the fact that the very serious inferences about mental deterioration in Germany at large, based on such quotations from German professional journals, seem indicated—*"even without actual frequency ratios*—in terms of *all* German professional articles *abstracted or published* (my italics) during the period" (1, p.

[1] The manuscript for this article, and an accompanying letter addressed to the Editor, were found among Dr. Lerner's papers by Mrs. Lerner, who forwarded them to the REVIEW.—Editor.

† Deceased.

179). As I explicitly stated, "let the reader ask himself what he would think of the state of public mental health in the United States, England or Switzerland . . . —*if* he could find such and similar statements in professional journals like the *American Journal of Psychology*, the *British Journal of Educational Psychology*, the *American Journal of Psychiatry, Mental Hygiene*, or their equivalents in different cultures" (as rough equivalents of the various psychological and *psycho-technical* publications [italics mine] cited in Section B) (1, p. 181). And furthermore, "let him ask himself what he would think even if only *one* (italics mine) such article appeared per year in each of the professional journals mentioned above" (1, p. 181).

3. In the light of the facts cited under No. 1 and No. 2, it is now clear that Wyatt and Teuber's contention that my selection tends to be 'misleading' is unwarranted and invalid. I would be certainly curious and grateful to learn just how my own original statements about the possible problems of selection in relation to analysis and interpretation of the data could be made *more* careful, cautious, objective—and *less* 'misleading'! But, in addition to being invalid, the authors' treatment of my study is *misleading* insofar as it can unwittingly befuddle the whole question about the likely extent of confusion and regression in German public thinking, and, therefore, about the probable magnitude of the task of German re-education. Here they weaken some of their own very suggestive conclusions about the "analysis of people who, under the impact of affective intoxication and fear are kept in a state of mass behavior, and of whom some happen to be psychologists" (2, p. 242). This is clearly indicated in certain additional statements by the authors.

Time and again it is implied or clearly suggested by Wyatt and Teuber that distinction ought to be made between Nazi and German psychology during the period in question. Thus, "in the three technical journals of rank there were, up to 1939, a number of papers which ought to be judged in terms of German rather than Nazi psychology" (2, p. 239). Now this may be a quite plausible hypothesis or inference—but not for the rather unrealistically naive reasons advanced by these investigators. For in this connection Wyatt and Teuber content themselves with emphasizing repeatedly that the papers "which bear no marks of the new spirit" are those published in the fields of physiological and experimental psychology—or, as they put it, "in the more impersonal, objective fields of psychology" (2, p. 230). Conversely, "the more philosophical a particular research, the more marked was the impact of NS [national-socialistic] ideology" (2, p. 230). These 'philosophical' fields of psychology turn out to be such specialties as child psychology, social psychology, psychology of personality, educational psychology, abnormal psychology and psychotherapy—or, as the authors put it, "those fields of psychology concerned with the common problems of the individual personality and of the group" (2, p. 242).

Very naturally one would not expect aberrations of feeling and thought to be as directly reflected in the field of psychophysics, as, say, in the fields most directly bearing on the very 'pattern of living' which, as Wyatt and Teuber themselves recognize, has become "increasingly emotionalized and regressive" and in which "the restraint and realism of scientific procedures becomes a danger as well as an offense for the individual who wants to sustain his indulgences at any cost and therefore can not permit his rationalizations to be questioned" (2, p. 242). This kind of reasoning—that biochemistry and psychophysiology

have not yet been Nazified, even if social psychology, child psychology, educational psychology, the psychology of personality *are* reflecting the new regressive mass mentality—might be in a sense but a tautologically over-elaborated case of begging the question if it did not, however unwittingly and well-meaningly, lend itself to further confusing the serious problem of German re-education after the war. It is notorious that trained chemists, engineers, physicians and other professional and scientific workers have become very active parts of the Nazi movement and the new regressive mass mentality. One should not judge the extent of their psychosocial deterioration in terms of what they are doing in organic chemistry (perhaps working on problems of super robot-bombs), but in certain other terms, perhaps of social psychiatry and mental hygiene, social and abnormal psychology. Likewise, one cannot possibly gauge German psychology under the Nazi system, even in terms of so-called pure scientific psychology, by naively stressing what is being done in psychological optics and acoustics. Scientific workers in such psychophysiological fields may do perfectly fine and objectively verifiable work and still be subject to the 'affective intoxication' of the period. To say or imply that what they do in physiological psychology is *German* psychology, regardless of what they feel and think privately as individual personalities, is to charge doubly to the ledger of what Wyatt and Teuber call *Nazi* psychology when it comes to the professional workers and writers in social psychology, child psychology or the psychology of personality (2, p. 239). It is simply that the specialties—and, therefore, writings—of the latter reveal certain emotional and all-round personality changes more readily than do those of the former.

Obviously the distinction to be made is not between 'Nazi' and 'German' psychology. What these investigators mean has to do with valid, objectively verifiable studies as against 'findings' clearly reflecting the 'affective intoxication' and 'mass behavior' produced by "the basic irrationalism of the NS movement" which, according to Wyatt and Teuber, "permeated all aspects of German life" (2, p. 241). Their loose terminology could easily lend support, without any real evidence, to the notion that the extent of 'affective intoxication' is not as great as it might appear from an examination of the work of psychologists in the less 'purely' scientific fields—simply because there is still a considerable amount of objective work done by psychologists in the more 'purely' scientific fields of physiology or biochemistry. Their equating of 'objectively verifiable' with 'German psychology' invites such misapprehension and is seriously misleading at a time when so much confused and confusing thought is being mobilized, pro *and* con, in the whole matter of 'Nazis versus Germans.'

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